PRESCHOOLERS ENROLLED AND MOTHERS AT WORK?

THE EFFECTS OF UNIVERSAL PRE-KINDERGARTEN

by

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Abstract

Three states (Georgia, Oklahoma and Florida) recently introduced Universal Pre-Kindergarten (Universal Pre-K) programs offering free preschool to all age-eligible children, and policy makers in many other states are promoting similar policies. How do such policies affect the participation of children in preschool programs (or do they merely substitute for preschool offered by the market)? Does the implicit child care subsidy afforded by Universal Pre-K change maternal labor supply? I present a model that includes preferences for child quality and shows the directions of change in preschool enrollment and maternal labor supply in response to Universal Pre-K programs are theoretically ambiguous. Using restricted-access data from the Census, together with year and birthday based eligibility cutoffs, I employ a regression discontinuity framework to estimate the effects of Universal Pre-K availability. Universal Pre-K availability increases preschool enrollment by 12 to 15 percent, with the largest effect on children of women with less than a Bachelor's Degree. Universal Pre-K availability has little effect on the labor supply of most women. However, women residing in rural areas in Georgia increase their children's preschool enrollment and their own employment by 22 and 20 percent, respectively, when Universal Pre-K is available.

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I. Introduction

Publicly subsidized Pre-Kindergarten (Pre-K) programs have received considerable attention in recent years as an avenue for providing child care and promoting school readiness. In Virginia, Governor Timothy Kaine campaigned on the promise of free preschool for all four years olds, while in New York, Governor Eliot Spitzer called for the "universal" program already in place to be made available statewide (Hakim 2007; Glod 2005).[1] More recently, as part of her presidential campaign, Senator Hillary Clinton unveiled a plan to provide up to $10 billion of federal funds per year to states that offer Universal Pre-K.[2] While economists have studied the effects of Pre-K interventions on the cognitive and non-cognitive development of participating children (Heckman and Masterov 2004, Gormley and Gayer 2005, Fitzpatrick 2007), the extent to which availability of Universal Pre-K increases preschool enrollment or affects the labor supply behavior of mothers is unknown.

Using birthday and age information coupled with eligibility cutoffs in a regression discontinuity framework, I examine how the availability of Universal Pre-K affects the enrollment in preschool of four year olds and the labor supply of their mothers. The two Universal Pre-K programs I study, in Georgia and Oklahoma, provide direct subsidies to child-care centers selected by parents for age-eligible children.[3] To enroll in either state's program, a child must turn four by September 1st of the school year in which they wish to enroll. The identification of intention-to-treat effects of Universal Pre-K comes from the exogenous difference in the eligibility of otherwise identical children born just before and after the cutoff.

[1] The state of New York introduced a plan for a Universal Pre-K program in 1997. When the program began, the plan was to roll out Pre-K over several years, starting in the poorest districts of the state. However, the program was never fully funded and therefore has never become available to all children in New York. Governor Spitzer has discussed providing the funding to make the program available statewide.
[2] http://www.hillaryclinton.com/news/release/view/?id=1743
[3] Florida introduced its Universal Pre-K program in 2005, after the collection of the data I use.

Restricted access Census Bureau data containing date of birth information make this identification strategy possible.

Universal Pre-K increases statewide preschool enrollment by 12 to 15 percent; these results are statistically significant and robust. Women with less than a Bachelor's Degree are even more likely than the general population to enroll their children in preschool in response to Universal Pre-K availability. For example, Universal Pre-K availability increases the preschool enrollment of children whose mothers have completed some college (but are not college graduates) by about 18 percentage points, or 25 percent. These findings are important, as research shows that disadvantaged children gain the most from early exposure to high-quality preschool programs (Fitzpatrick 2007, Gormley and Gayer 2005). However, Universal Pre-K availability has no discernable effect on the preschool enrollment of children whose mothers have at least a Bachelor's Degree, perhaps because the preschool enrollment rate of the children of these mothers is over 75 percent without Universal Pre-K, which is anywhere from 10 to 30 percentage points higher than that of their less educated counterparts.

In addition, the results indicate no robust impact of Universal Pre-K availability on maternal labor supply. However, minor changes in labor supply are observed for certain groups of mothers. For example, mothers living in rural areas are 10 percentage points or 20 percent more likely to be employed because of Universal Pre-K availability. Use of a fuzzy regression discontinuity framework to estimate the local average treatment effects of preschool enrollment on maternal labor supply for women whose children are born close to the eligibility cutoff confirms the general finding that Universal Pre-K has no discernable effect on labor supply. Such results are consistent with recent findings that female labor supply elasticities are smaller now than they once were (Blau and Kahn 2007, Heim 2004). The theoretical model I outline

incorporates preferences over child care quality to help explain why maternal labor supply might not be as responsive to government subsidization as it is traditionally thought to be.

Section II reviews the institutional details of Universal Pre-K, presents a theoretical model to motivate the empirical methods and summarizes existing evidence about the relationship between child care and maternal labor supply. Section III provides a description of the empirical methods and the unique data used in the analysis. Section IV presents the results and several specification checks. The conclusion follows in Section V.

II. Universal Pre-K Programs & Previous Evidence

II.a. Institutional Details

In 1993, Georgia instituted a lottery to fund the HOPE scholarship and a pre-kindergarten program for four year olds.[4] Both were initially available only to low- and middle-income households, but the programs were expanded two years later to include all age-eligible state residents. In the 2004-2005 school year, approximately 55 percent of four year olds were enrolled in Georgia Pre-Kindergarten (GPK) at a total state cost of $276 million. In 1980, Oklahoma began a rationed pilot Pre-K program providing care to a small number of four year olds at no cost to parents. In 1990, the program expanded to include all Head Start eligible four year olds in the state. In 1998, Oklahoma further expanded its Early Childhood Program for Four Year Olds (ECPFYO) to include all age-eligible children regardless of income. By the 2004-2005 school year, enrollment in the program reached 68% of four year olds and cost $80 million.[5] Figure 1 details how enrollment in these Universal Pre-K programs grew. Both

[4] The HOPE scholarship has received much more attention from politicians and economists than its sister program, Georgia Pre-K. For examples, see Dynarski (forthcoming), Dynarski (2000) and Long (2004).
[5] http://nieer.org/yearbook/pdf/yearbook.pdf (March 13, 2007)

programs were well-implemented at the time of data collection, though Oklahoma's program continued to grow after 2000.[6]

Both Georgia's GPK and Oklahoma's ECPFYO are voluntary, free, and available to all children who turn four by September 1, regardless of family income. In both states, a wide range of approved facilities, including public schools, Head Start centers, private child care centers, faith-based centers and other non-profit centers can provide Pre-K. Programs in both states run five days a week for the length of the school year, but Georgia mandates a 6.5 hour day while Oklahoma offers both half- (2.5 hours) and full- (6 hours) day options.[7] Teachers and classroom assistants must meet educational requirements higher than those for non-Universal Pre-K centers in both states. Both programs impose a minimum staff to child ratio of 1:10 and a maximum of 20 students per classroom. In Georgia, providers may choose to follow one of several approved curricula, while in Oklahoma there are curriculum "guidelines."[8]

The statutory incidence of both subsidies is on the firm side. The state of Georgia transfers lottery funds directly to centers. In 2004-2005, expenditures per child averaged $3,889, though actual reimbursement rates varied slightly based on location and teacher education. In Oklahoma, public school districts receive money from the general revenue allotted for the program on a first-come first-served basis. The districts may then provide the service themselves or contract out to child care centers that meet the state requirements. In the 2004-2005 school year, expenditures averaged $2,517 per child, though actual reimbursement rates depended on

[6] Perhaps this is a reason the estimated effects of Universal Pre-K for Oklahoma are not as large as those for Georgia.

[7] This difference in program lengths may be another reason the estimated effects of Universal Pre-K in Oklahoma are not as large as those in Georgia. Both options in Oklahoma are free to parents but reimbursement rates to providers depend on the length of care provided. Both states encourage centers to offer additional care (after set program hours and during the summer). However, neither pays the cost of this "supplemental" care.

[8] All of these regulations are stricter than those for non-Universal-Pre-K child care centers. For example, centers in Georgia not receiving state money for Universal Pre-K must have a staff to child ratio of at least 1:18, a maximum group size of 36, and there is no minimum educational requirement for teachers or assistants.

whether children attended full- or part- day sessions.[9] To place the size of the subsidies in context, average child care costs for full-day center care for four year olds in the U.S. ranges from $3,900 to $10,200.[10] Although Universal Pre-K does not provide full workday care, by caring for children for at least part of the day it provides a substantial subsidy for child care to families with four year olds.

While Georgia and Oklahoma were the only states providing Universal Pre-K in 2000, many other states had targeted Pre-K programs, and the federal government provided funds for Head Start, another means-tested preschool subsidy for four year olds. Forty-three states funded targeted Pre-K programs in the 2001-2002 school-year.[11] Including children in Georgia and Oklahoma, almost 15 percent of four year olds nationwide were enrolled in state-funded Pre-K programs in 2001-2002. Head Start enrolled another 12 percent of four year olds.[12] The counterfactual of this study therefore includes such means-tested Pre-K programs in existence in 2000. Thus, the question answered here is: "What effects does Universal Pre-K have on preschool enrollment and maternal labor supply compared to existing early childhood education subsidy programs?"

II.b. Key Implications of a Model of Time Use and Child Quality

Consider the traditional two good model used to describe mother's child care and labor supply choices (Gelbach 2002, Cascio forthcoming). In it, the mother can choose between working (and purchasing care) or leisure (which implicitly includes taking care of the child

[9] http://nieer.org/yearbook/pdf/yearbook.pdf (March 13, 2007). Centers cannot receive more than a small registration fee from parents in either state.
[10] http://www.naccrra.org/randd/ (October 25, 2007)
[11] I report 2001-2002 school-year data because it is the year closest to 2000 for which data are available. http://nieer.org/yearbook2003/pdf/yearbook.pdf#page=6 (October 25, 2007)
[12] http://www.acf.hhs.gov/programs/hsb/about/fy2003.html (October 25, 2007)

herself). In this two good framework, a Universal Pre-K subsidy provides a full price subsidy for child care on the margin for any woman working less than the length of the Universal Pre-K program. This price subsidy provides incentive for these mothers to enter work or increase the number of hours they work. For women who work more than the length of the program day in the absence of Universal Pre-K, the program provides an income subsidy. This income subsidy exerts downward pressure on the amount of time in the workplace for these women. Crucially, this model does not allow for maternal preferences concerning the inputs into her child's development. Because the mother's preferences only include leisure and consumption, an actual estimated effect of Universal Pre-K based on this model will only depend on the relative sizes of these price and income elasticities in the population and the distribution of number of hours worked by mothers of four year olds in the absence of the program.

The model I present in the Appendix and outline here is a three good model where a mother has preferences over consumption (*X*), leisure (*L*) and child care quality (*Q*). The amount of time the child spends with the mother (C_m) and in the care of others (C_o) determines total child care quality: $Q = \theta C_m + C_o$, where θ is the relative quality of the mother's care. The mother can spend her time at work (*H*), in leisure (without her child) or caring for her child. A Universal Pre-K program enters the model as a fixed amount (u) of care by others offered at no charge to the mother that is the same quality as existing market care. Figure 2 presents three-dimensional graphs showing a mother's budget frontiers with and without Universal Pre-K. Time spent in leisure, quality of child care and consumption are on the axes.[13] Panel A depicts the mother's budget frontier absent the program. Panel B depicts the frontier in the presence of

[13] The figures are drawn for a woman facing the utility maximization problem outlined in the appendix whose own quality of child care is better than the quality of care by others ($\theta > 1$). For more detail and an explanation of the model see the Appendix.

the program. Part of the budget frontier – corresponding to amounts of child care smaller than *u* – is the same in each situation. But in Panel B with take-up of the program providing *u* units of care by others, part of the budget frontier shifts vertically upward by the amount *u* times *p*. As in the simpler model, Universal Pre-K provides an income subsidy to some women and a price subsidy to others, depending on their location on the budget constraint, but the mother's decisions will also depend on the relative quality of market care to the child care she provides herself.

In this model, both leisure and consumption financed by working require the use of paid child care, so the cost of switching from one hour of leisure to an hour of work is *w*, regardless of Universal Pre-K availability. Thus the only price subsidy in this model is for women who without Universal Pre-K spend a more than 1-*u* hours caring for their own children (e.g. point A, Figure 2.A). For these women the subsidy provides an effective "price cut" for *either* labor supply or leisure, because under Universal Pre-K these activities no longer involve child care costs. However, there is also a quality dimension to the decisions of mothers in this model. The quality of care by others (including Universal Pre-K) is not necessarily the same as the quality of maternal care. As a result, for a woman who spends a good deal of time with her children, the estimated effect of Universal Pre-K will depend on her price and quality elasticities of labor supply and child care use, which in turn depend on the price of child care relative to her wage and the quality of her maternal child care relative to the quality of market (and Universal Pre-K) care. Women who have strong preferences for high quality child care, are relatively better at taking care of their children than the market and are relatively better at taking care of their children than they are at earning money will not increase their labor supply because of this subsidy. For example, some women at point A do not change their behavior with the

introduction of Universal Pre-K. In the two-good model, the only explanation for these women to not increase labor supply was that they had a high reservation wage. My model offers a different potential explanation – the mother does not increase her labor supply in response to Universal Pre-K because of her preferences for high quality child care.

In this three good model, Universal Pre-K will be an income subsidy for women who are spending less than *u* hours in care of their child and hence either a) working more than *u* hours, b) in leisure more than *u* hours or c) in some combination of labor and leisure for more than *u* hours. For women in group (a), the income subsidy puts downward pressure on the amount of time spent working, increasing these mothers' own time in care of their children (by spending more time during non-preschool hours with them) and in leisure, but still allows more consumption, if these things are all normal goods.[14] An example of this type of woman would be one who moves from point C in Figure 2.A. to point E in Figure 2.B. Point E is on a vertically higher plane than point B, so the woman has more consumption, but she also gets more leisure and quality of child care than she did without Universal Pre-K. Women in groups (b) and (c) also face the income subsidy of Universal Pre-K. The effect of this income subsidy exerts downward pressure on their labor supply. For women not working in the first place (Point B), there is no change in labor supply (move to point D). However, some of these women were working and will work less (such as those that move from point F to point D).

In contrast to the two good model, several elasticities will govern the response to the Universal Pre-K subsidy in this model: the elasticities of labor supply, leisure and child care use with respect to the price of child care; the elasticities of labor supply, leisure and child care use with respect to income; and the elasticities of labor supply, leisure and child care use with

[14] See the comparative statics in Appendix 1. Most jobs offer a set number of hours. As is discussed in the next section about extensions to the model, this rigidity of hours might prevent women from changing their labor supply because of Universal Pre-K introduction.

respect to the quality of child care. The estimated effect of Universal Pre-K in this model will depend on the budget parameters, the joint distribution of these elasticities and the distribution of time use in the absence of the program (which also depends on these elasticities). Without knowledge of the balance of these in the population, theoretical predictions about the effect of Universal Pre-K on program take-up and maternal labor supply are ambiguous. Before turning to the empirics to determine the effects of Universal Pre-K, I first describe how incorporating some real world extensions to the model just described might affect its theoretical predictions.

Implications of model extensions

Some important extensions to the model may influence the interpretation of the empirical results. I discuss them here briefly in turn:

- If labor demand is not perfectly elastic, the woman may not be able to change her time spent working to fit her preferences. For many women, this is likely to be the case as most jobs require full or part time participation or at least entail a set number of hours per shift. This rigidity of number of hours of work available may be one reason I see little change in the labor supply of mothers in response to Universal Pre-K.

- Similarly, if the supply of child care is inelastic, it may not be possible for women to supplement Universal Pre-K with additional care at the market price. Universal Pre-K does not provide full workday care for women with full time jobs working eight hours a day. If there is a sizeable cost to placing the child in supplemental care at a Universal Pre-K center, the mother may not take up the program. Therefore she will not change her labor supply.

- If the mother has other children under school age, the cost of both working and leisure is higher than for a mother with only a four year old because either requires that she pay for child care for her other children. This will make Universal Pre-K less valuable, reducing the likelihood of take-up and any change in labor supply.

- A mother may have more than one resource for providing child care. There are several varieties of child care for four year olds, e.g. preschools, day care, babysitters, and family members, each of which provide different levels of child care quality and charge different prices. Universal Pre-K care may be of better or worse quality than care purchased in the market. The set of options means that the mother's elasticity of child care use with respect to quality and price become even more important in her decisions about the use of child care and labor supply. The predictions of the model become even more complex

depending on relative qualities of the various types of care (including the mother's) and their prices.

- In addition to the quality of time resources devoted to children, quality of child production might depend on dollar resources spent on children. Buying books to read with children is an example of how dollar resources can be used to supplement and/or substitute time resources. The model could be extended to include financial resources in the production function for quality, which would lead to mothers having different preferences over the consumption of the family and the consumption devoted to child quality. A result of this type of extension might be that mothers funnel the extra money provided by Universal Pre-K into child quality production, rather than use it to increase their leisure.

Each of these extensions complicates the analysis of potential effects of the program, but the result remains that the effects of this type of subsidy are theoretically ambiguous, and so I turn to empirics to examine the effects of Universal Pre-K. As I describe my results, I will allude to the model once again in discussing what the results imply about the underlying responses. I will distinguish among people who might be expected to differ in their responses along the lines suggested here (i.e., because they differ by marital status, education, and urban/rural location, characteristics which are likely to be correlated with non-labor income, market wages, and child care prices).

II.c. Related Evidence: Subsidization and Preschool Enrollment

Reports from Georgia and Oklahoma indicate enrollment in their Universal Pre-K programs between 50 and 60 percent of all four year olds (Figure 1) - program take-up is high. But increases in Universal Pre-K enrollment do not necessarily represent increases in preschool enrollment. It may be that the children attending the Universal Pre-K programs would have attended other preschool programs in the absence of Universal Pre-K, raising classic questions of crowd-out from publicly provided services (seen most frequently in discussions of health

insurance, e.g. Cutler and Gruber 1996). However, very little evidence exists about whether crowd-out is an issue in the child care industry. None exists about the crowd-out effects of Universal Pre-K in particular. Additionally, the universality of the program makes it unlikely that eligible families will respond as they do to the widely studied similar but targeted programs, such as Head Start or Perry Preschool. Higher income families are more likely to be using preschool in the absence of a subsidy making it less probable that universal programs induce any change in behavior above that of targeted programs.

Whether Universal Pre-K improves children's academic outcomes more than alternative modes of child care probably influences whether families enroll their children. Gormley and Gayer (2005) and Gormley, Gayer, Phillips and Dawson (2005) analyze the effect of participation in the Oklahoma Pre-K program on test scores of students in Tulsa. In order to attempt to control for potential selection bias, they compare test scores of children just finishing Pre-K to those just entering it. The underlying assumption is that both groups are comparable on unobservable characteristics since they have all chosen to participate in the Universal Pre-K program. The researchers find that Oklahoma Pre-K participation increases test scores by 0.24 to 0.39 standard deviations, depending on the test subject. In a differences-in-differences analysis of the intention-to-treat effects of Universal Pre-K on achievement scores of fourth graders, Fitzpatrick (2007) finds that the availability of Georgia Pre-K increases the math and reading scores of school lunch eligible children (0.07 and 0.03 percent of a standard deviation, respectively).

II.d. Related Evidence: Child Care Subsidies and Maternal Labor Supply

Female labor force participation has changed dramatically in recent decades, fostering interest in the role of children in female decisions about work. Within the last fifteen years, investigators have used both demonstration programs (such as the New Chance program) and widespread targeted subsidies to examine the relationship between child care subsidization and maternal labor supply. Researchers consistently find evidence that subsidization of child care increases maternal labor supply (Bos et al. 1999, Granger and Cryton 1999, Berger and Black 1992, Blau and Tekin 2003).[15] However, more recent analyses of the elasticity of female labor supply show women are no longer very responsive to wage changes (Blau and Kahn 2007, Heim 2004). This decreased responsiveness to wages might mean child care subsidies will have less impact on maternal labor supply today than in the past.

The presence of variants on "universal" preschool subsidies in other countries and for children of different ages provides further opportunity for measurement of the effects of child care subsidization on labor supply. Baker et al. (2005) study a program providing child care for an out-of-pocket price of $5 per day (of any length) to all children under five years old in Québec, Canada. The researchers use a differences-in-differences approach, comparing the labor supply of mothers across Canadian provinces before and after the program began. The authors find a statistically significant and sizeable increase in employment of married mothers of 7.7 percentage points. Schlosser (2005) studies the introduction of free compulsory public preschool in Israel for children ages three and four. She uses variation in the timing of program introduction across localities to identify the effects of the program on maternal labor supply. She

[15] The analyses of the widespread subsidies are biased if the measures used to control for selection (e.g. waiting lists, instruments) are invalid. Some of these experiments involve random assignment and therefore typical selection bias problems do not contaminate estimates of treatment effects. However, because these studies are geographically and socio-economically concentrated and have small sample sizes, the results may not generalize to larger and/or more diverse groups. In addition, many of these programs included packages of services and treatments along with the child care subsidy. This makes it difficult to disentangle the effects of the subsidy alone.

also finds effects of about 7 percentage points. My research differs from these studies in both the type of subsidy considered and the population served. Moreover, the identification strategies could be problematic if shocks specific to the areas during the periods the researchers study are related to the policy change. Here, the use of age cutoffs as an additional source of identification alleviates this potential problem because any shock in Georgia or Oklahoma to mothers of four year olds likely has similar effects on those born both before and after September 1st.

In the work most closely related to this study, Gelbach (2002) uses quarter of birth as an instrument for enrollment in kindergarten in 1980. He then estimates the impact of this large implicit child care subsidy on the labor supply of mothers in the U.S. He finds evidence of a 6-24 percent increase in labor supply measures.[16] In a slightly different approach, Cascio (forthcoming) uses the timing of kindergarten introduction (which largely occurred in the 1960s and 1970s) as an instrument for kindergarten participation in the estimation of the effects of kindergarten enrollment on maternal labor supply.[17] She shows evidence of an increase in maternal labor supply of single mothers without other young children due to the increased funding of kindergarten, but no effects for other groups of women. In contrast to these two papers, I focus on Pre-K. The distinction is important as societal convention may make it more likely that a mother enroll her children in kindergarten than in preschool. Also, the years the other authors study precede the new evidence that women are not responsive to wages. Finally, the precise birthday information in the unique data set I use here allows for differentiation

[16] If it is the case that Universal Pre-K affects maternal labor supply, it might also decrease the dependency of some women on government assistance. Gelbach (2002) also examines the effects of kindergarten enrollment on the receipt of public assistance of mothers in 1980. Although he finds that kindergarten reduced public assistance receipt by 10 percent, the increased participation of mothers in the workforce in recent decades, coupled with more stringent welfare laws enacted in the 1996 Personal Responsibility and Work Opportunity Reconciliation Act, might make the mothers receiving public assistance income in 1999 quite different from those doing so in 1979.

[17] In another related paper, Cascio (2005) uses variation in the funding of kindergarten programs as an instrument to control for selection bias in a study of the effects of increased kindergarten access on children's longer-term academic outcomes. She finds that the programs decreased grade retention between 20 to 40 percent but had little effect on high school graduation.

between the enrollment effects of child development and of legal rules, providing a distinct improvement on methodologies relying on quarter of birth.

III. Methods

III.a. The Regression Discontinuity

$Y_i(1)$ and $Y_i(0)$ represent the outcome if an individual, *i*, is eligible or ineligible for Universal Pre-K, respectively. In cross-sectional data, it is impossible to observe individuals at points in time when they both did and did not receive treatment, so researchers typically examine differences between average outcomes for groups of individuals who did and did not receive treatment. Defining the treatment individuals receive as W_i, this difference is $E[Y_i \mid W_i = 1] - E[Y_i \mid W_i = 0]$. If the assignment to treatment is random, this difference represents the causal effect of treatment on the outcome. In regression discontinuity analyses, the value of some observable characteristic (D_i) determines treatment status, such that $W_i = 1\{D_i \geq d\}$.

Focusing on within-state variation in Universal Pre-K eligibility, children in Georgia and Oklahoma born on or before September 1, 1995 differ from those born on or after September 2, 1995 in that only the former are eligible for Universal Pre-K in the 1999-2000 school-year. Hahn, Todd and van der Klaauw (2001) term this a *sharp* regression discontinuity design: a child's date of birth completely determines the treatment, *eligibility* for Universal Pre-K. The effect of Universal Pre-K eligibility on an outcome, such as enrollment in preschool, can therefore be estimated by

$$\Delta = \lim_{d \downarrow 0} E[Y_i \mid D_i = d] - \lim_{d \uparrow 0} E[Y_i \mid D_i = d].$$

Y_i is the dependent variable of interest for mother *i*, D_i represents the day on which mother *i*'s child turns four and *d* represents September 1st. The underlying assumption is that the relationship between birthdate and preschool enrollment would be smooth through the cutoff (September 1st) were it not for the Universal Pre-K program. This identification strategy assumes that mothers are not able to manipulate their children's eligibility, much as in a randomized controlled trial in which participants are not able to change their assignment to treatment group. Also similar to a randomized controlled trial, there should be no differences between the demographic characteristics of mothers whose children are in either "treatment" group, i.e. between those who are eligible and those who are not.

The model for the estimation of the treatment effect is

$$Y_i = f(Days_i) + \Delta cutoff_i + \upsilon_i .$$

Here, *Days*, the number of days older than a specific date the child is, is a function of the child's date of birth. Eligibility is given by the discrete variable *cutoff*.[18] The regression discontinuity literature uses two types of estimators to estimate Δ: the local polynomial and the flexible parametric model. In what follows I choose to use the flexible parametric model. One reason is general familiarity with this type of estimation. Another is the discrete support of birthdays which arguably makes it difficult to get arbitrarily close to the cutoff date (without time of birth).[19] I use a quartic, i.e. $f(Days_i) = \sum_{j=0}^{4} \pi_j Days_i^j$, and continuing to focus on within-state

[18] The *Days* variable is measured in relation to March 1, 1995. In order to participate, children enrolled in Universal Pre-K in the 1999-2000 school year would have to have been born by September 1, 1995. Take the example of a boy born on August 15, 1995. If he lived in Georgia, this child was eligible for GPK and would therefore have a value of $cutoff_i$ equal to one. If, however, he lived in Indiana, his value of $cutoff_i$ would be zero. Regardless of where the boy lives, his value of $Days_i$ is 167. Another child, born on September 15, 1995, would have a value of $Days_i$ equal to 198. In either Georgia or Indiana, this child would have a zero for the $cutoff_i$ variable. In some other states with later cutoffs, such as North Carolina, the child would have a value of $cutoff_i$ equal to one.

[19] Because this point is debatable, I also have done the analysis using local polynomial methods. The results are qualitatively the same.

variation in eligibility, the equation becomes $Y_i = \alpha + \beta X_i + \sum_{j=0}^{4} \pi_j Days_i^j + \Delta cutoff_i + \varepsilon_i$. The inclusion of demographic characteristics in X_i is not necessary given the identification strategy, but allows for variance reduction.

Although only two states have Universal Pre-K, every state in the U.S. mandates that some type of kindergarten be made available for their residents; almost all states set restrictions on the age of children enrolled. This eligibility restriction for kindergarten in other states further helps identify the effects of Universal Pre-K. Because the eligibility dates in Georgia and Oklahoma for kindergarten and Universal Pre-K are the same, without the use of other states it would not be possible to distinguish between enrollment effects of Universal Pre-K and enrollment effects resulting from a child's eligibility for kindergarten in the following year. Consider the extreme case where, regardless of Universal Pre-K availability, a family would have decided to send their child to preschool in the year before he or she will be age-eligible for kindergarten. Without controlling for the kindergarten eligibility that also comes with the September 1st cutoff, I would inappropriately attribute this child's preschool enrollment to Universal Pre-K.[20] Incorporating states without Universal Pre-K, the estimation equation is the following:

$$(1) \quad Y_i = \alpha + \beta X_i + \gamma State_i + \sum_{j=0}^{4} \pi_j Days_i^j + \theta cutoff_i + \delta_1 GA_i \times cutoff_i + \delta_2 OK_i \times cutoff_i + \varepsilon_i .$$

$State_i$ represents state fixed effects that control for fixed differences across states in outcomes. The variable $cutoff_i$ has a value of one if the mother's child was of age before the cutoff date in

[20] It should be noted that although state legislatures have mandated that school districts offer kindergarten, most states do not require kindergarten attendance. A handful of states – Colorado, Illinois, Massachusetts, New Hampshire, New Jersey, Pennsylvania, and Washington – allowed local municipalities to set the minimum entrance age for their school districts at the time of the 2000 Census. These states are not included in this analysis.

his or her state (in time for Universal Pre-K in the fall of 1999 or kindergarten in the fall of 2000). The interaction term $GA \times cutoff_i$ ($OK \times cutoff_i$) will have a value of one for mothers whose children in Georgia (Oklahoma) are old enough to participate in Universal Pre-K. The effect of Universal Pre-K on the dependent variable is then measured by δ_1 and δ_2.[21]

Identification of program effects comes from variation in behavior within states, for different ages of children and by different cutoff dates, specifically among those who are induced to change their behavior because of their treatment status. This *sharp* regression discontinuity framework therefore identifies the local average intention-to-treat effect of Universal Pre-K. However, without additional assumptions or data, this estimation will not provide information about the mechanisms through which Universal Pre-K might affect these outcomes. Increases in preschool enrollment might occur because of an in-kind transfer of child care, as in the model described earlier. Using a different model, increases in preschool enrollment may occur because Universal Pre-K provides much higher quality child care than other available care.

III.b. Restricted Access 2000 Decennial Census Data

To conduct this analysis, I use the Census Decennial Long Form Restricted Access Data. The data generally comprise a one-in-six sample of the population of the United States surveyed in April 2000 and include demographic, labor force participation and educational enrollment information about survey respondents and other members of their households. The information provides a set of variables, *X*, about the mothers to use as controls in the analysis. The set consists of age and education, gender of the child, race, a set of dummy variables for whether the

[21] There are two separate interaction terms because, as detailed earlier in the paper, the programs in Georgia and Oklahoma differ on important parameters that might cause maternal responses to their introduction to differ. It is also important to note that because non-Universal Pre-K states have a range of kindergarten cutoff dates (from June 1st to January 1st, including non-program states with September 1st cutoff dates), the estimates of the effects of Universal Pre-K do not include information about the importance of the date itself.

family lives in a central city, rural area or urban fringe, state of residence, and the number and ages of other members of the household.

The primary outcomes of interest are enrollment of the child in preschool as of February 2000, mother's employment in the week prior to the survey, and mother's employment, weeks of work, usual hours, wages, and family's public assistance receipt in 1999.[22] When the dependent variable is binary, e.g. preschool enrollment, I use probit estimation and report marginal effects.[23] In 2000 the Census Bureau asked for the date of birth of respondents and other household members; this is crucial information for identification of program effects. Combined with data collected from the states on kindergarten cutoffs in 1999, as shown in Table 1, this information identifies program effects as described above.[24]

In order to create the sample for the analyses, I limit the entire sample to the subset of mothers who live with their own children who were born in the one year period between March 1, 1995 and February 28, 1996, so that there are six months worth of eligible children and six months worth of ineligible children. Of this sample, I dropped the mothers with multiple children born on the same day, more than four other adults in the same household and more than two of their own or step-children over 18 years of age because the parameters they face are likely very different than those of other mothers (though the results are not sensitive to these restrictions). Together these groups account for less than 3 percent of the sample of mothers of

[22] The enrollment question asks whether the child was enrolled in school as of February 2000. If a respondent answers yes, he/she is then asked what level of school the child is attending, where one of the options is preschool or nursery school. The labor supply questions are intended to refer to all of 1999, while the program would have only affected the mothers of four year olds in the last four or five months of 1999. This means that estimates of program effects on the labor supply decisions may underestimate the impact of full participation in Pre-K.

[23] Reported here are the marginal effects at the mean for continuous variables and for a change of zero to one in dummy variables. Average marginal effects are qualitatively similar.

[24] Data on kindergarten cutoffs were collected from a survey conducted by the Indiana Department of Education, found at http://www.doe.state.in.us/legwatch/2000/a_kinder_issues.html (Accessed October 2, 2006). In general, the cutoffs reported there conform to those for the same period from other sources, such as the Education Commission of the States.

four year olds. In addition, I drop observations for which data were missing. Most of the results in the following sections use a sub-sample of these four year olds in a narrower range of the eligibility cutoff. Using a width of 30 days on either side of the cutoff creates groups eligible and ineligible for the program that are quite comparable. The weighted number of observations in this two-month sample is 430,681, which corresponds to about 65,000 observations.

Table 2 presents the average characteristics for these mothers by state of residence. These characteristics show support for the assumption underlying the regression discontinuity framework. Mothers of children born within 30 days of a state's cutoff are essentially the same on observable characteristics – they are, on average, the same age and race, are equally likely to be married, and have the same amount of education. This is true for states with and without Universal Pre-K.

I also confirm the validity of the assumption underlying the identification strategy by examining the continuity of mothers' demographic characteristics on either side of the cutoff for enrollment. Figures 3.A.-3.E. shows the percent of mothers who are white, the percent who are married, the average age, the percent who have additional children younger than the four year old and the percent who have any other children. In each panel there are 3 lines, representing Georgia, Oklahoma and states without Universal Pre-K. The horizontal axis measures the age (in days) of four year olds relative to the cutoff dates in their states of residence. Children to the left of zero are born after the age cutoff date in their state and those to the right are born before the age cutoff date. Due to the confidential nature of the data, the graphs are smoothed splines of the relationships between the child's age relative to the cutoff and the other variables.[25] The

[25] To preserve the nature of the original data, the splines are minimally smoothed. An examination of the actual data in bins by the author within the Research Data Center produced very similar results.

graphs confirm that there are no differences between the exogenous characteristics of the mothers of children born on either side of the cutoff.

If mothers manipulate the eligibility of their children by altering their actual or reported birthdays, we might see an increase (or decrease) in the number of children born just before the cutoff date (McCrary 2007). Figure 4 presents the number of mothers whose children are born on any given day (relative to the cutoff date in their state of residence). These pictures also show no differences on either side of the cutoff. Figures 3 and 4 provide additional evidence that there are no differences (other than eligibility for Universal Pre-K) between women with four year olds born in a narrow range on either side of the enrollment cutoff date in their state.

IV. Results

IV.a. A Visual Look at the Effects of Universal Pre-K

An illustration of the relationship between the age of the child and enrollment in preschool shows the basic identification strategy. Figure 5.A. plots a smoothed spline of the relationship between the age of children and their rates of enrollment in preschool for groups in three locations: Georgia, Oklahoma and states without Universal Pre-K. Again, the horizontal axis represents the distance in days between children's birthdays and the cutoff date in their states. Preschool enrollment rates for children born in time for Universal Pre-K in the fall of 1999 or kindergarten in the fall of 2000 are to the right of the cutoff day and for those born after the cutoff date are to the left.

The diagram shows that, on average, being born before the cutoff is positively related to enrollment in preschool. For states without Universal Pre-K, the increase in enrollment rates for

those born before their cutoff is approximately 8 percentage points.[26] This can be construed as an increase in parents' willingness to send children to preschool at the age of four because they will be old enough to attend kindergarten the following year.[27] For children born in Universal Pre-K states the increase is even larger, approximately 19 percentage points for Georgia and 15 percentage points for Oklahoma. This effect can be construed as the combination of being able to enroll your child in Universal Pre-K in the fall of 1999 and kindergarten the following year. The difference between the two, 11 percentage points in Georgia and 7 percentage points for Oklahoma, represents the difference-in-differences estimate of the effect of Universal Pre-K (for those who are affected by the age restriction).

A similar picture of the relationship between the age of a child (relative to the cutoff date in his or her state) and maternal employment in the week prior to the survey is in Figure 5.B. No discernable relationship exists between a child being born before or after the cutoff date and a mother's probability of being employed. Though not presented here, other measures of maternal labor supply similarly show no relationship with the age cutoff.

IV.b. Estimation Results for Preschool Enrollment

Regression analyses confirm the visual relationship between Universal Pre-K availability and preschool enrollment. I find an average effect when I look at enrollment for all four year olds in the state and within most sub-groups of women expected to be differentially affected by Universal Pre-K. The first and second rows of Table 3 present the estimates of the effect of Universal Pre-K in Georgia and Oklahoma, respectively, by area of residence. Universal Pre-K

[26] The numbers used in this paragraph are from the information in Figure 2 and are not actual statistics from the data. Analysis of the data returned very similar results.
[27] This also captures any behaviors of preschool centers in other states showing preference to children able to attend kindergarten the following year.

increases preschool enrollment of four year olds in Georgia by 9.5 percentage points and in Oklahoma by 6.2 percentage points. Given the baseline enrollment of non-eligible children in each state, the estimates translate to an increase in preschool enrollment of 15 and 12 percent, respectively.

Turning to the demographic characteristics, white mothers, married mothers and mothers with other household members and children are all less likely than their counterparts (non-white, single or those with no other household members or children) to enroll their children in preschool. Older mothers and those with more education are more likely than younger or less educated mothers to enroll their four year olds in preschool.

As a reminder, *cutoff* is a dummy variable that takes a value of one if the child was born before the cutoff date for kindergarten in the fall of 2000 (and Universal Pre-K in the fall of 1999). Being of age for kindergarten at that point makes it 8 percentage points more likely that a four year old is enrolled in preschool in the spring of 2000, and the estimate is statistically significant at the one percent level (as are most of the other estimates). This 13 percent increase represents the additional willingness of mothers to enroll their children in preschool if the child will be eligible for kindergarten in the following year. Accounting for this increased willingness through the use of other states as a control group is vital for precisely estimating the effects of Universal Pre-K.

IV.b.i. Preschool Enrollment Differences by Geography

The cost of child care depends in part on one's location. For example, on average, the closest child care center in rural areas is farther from family homes than it is in urban areas, likely raising the fixed transportation costs involved with child care use in rural areas. The

second through fourth columns of Table 3 present the estimation results when the sample is restricted to rural, urban cluster or urbanized areas, respectively.[28] These classifications are based on a block group's population density as well as its proximity to other block groups of high population density. [29] Figure 6 maps these classifications in a few states to illustrate the divisions. Functionally, urban cluster represents large separate towns rather than the suburbs of big cities.

This distinction is important because of the differences in the estimates across these location types. Universal Pre-K has the largest effect in the less densely populated areas. The program increases preschool enrollment in rural Georgia by 11.6 percentage points and in rural Oklahoma by 9.9 percentage points, or 22 and 25 percent, respectively. Both estimates are statistically significant at the one percent level. In urban clusters, the estimated effects are 14.8 and 10.4 percentage point increases in enrollment for Georgia and Oklahoma, respectively, though only the estimate for Georgia is shown to be statistically significant. It translates into a 24 percent increase in preschool enrollment for children living in urban clusters. In urban areas in Georgia, Universal Pre-K is estimated to have increased preschool enrollment by 8 percentage points or 12 percent. In urban areas in Oklahoma, the estimate is essentially zero. The increases in urban areas are statistically smaller than the program's effects in rural areas or urban clusters. Such differences across location types suggest a supply side response to the Universal Pre-K program. Existence of "thin markets" for preschool is one potential explanation. There may not have been enough demand by families with four year olds in less densely populated areas to

[28] An alternative way to separate the sample for the following analyses is to use the whole sample and include interaction terms between eligibility and residential area (or marital status or educational attainment or the presence of younger children). Doing so, the results are not qualitatively different. The results from the pooled sample with interaction terms were used in determining whether differences in estimates of the effects of Universal Pre-K for mothers with different characteristics were statistically significant from each other.
[29] For definitions, see http://www.census.gov/geo/www/ua/ua_2k.html.

induce supply. Once the government raises demand by subsidizing preschool, firms enter the market and more children enroll.[30]

IV.b.ii. Preschool Enrollment Differences by Marital Status

Different mothers face different constraints when making decisions about the use of child care and the decision to work. In the context of the theory outlined earlier, married mothers likely have more "exogenous" income (if their husbands work) than single mothers. Also, mothers with more education on average receive higher market wages than those with less education. To see whether these differences translate into different adjustments to Universal Pre-K, I repeat the estimation of equation (1) separately for married versus single mothers and by levels of educational attainment – less than a high school diploma, exactly a high school diploma, some college attendance or a completed Bachelor's Degree, graduate or professional degree (Table 4). Some of the precision of previous estimates is lost as "cell sizes" become small for specific groups of women, particularly in Oklahoma. [31] However, the results show that Universal Pre-K availability induces new preschool enrollment of young children from most backgrounds, with the largest effect for children of women with lower levels of educational attainment.

Universal Pre-K in Georgia increases the likelihood that single mothers enroll their four year olds in preschool by 11.3 percentage points, or 16 percent, compared to 9 percentage points, or 13 percent, for married mothers. Both estimates are statistically significant at the one percent

[30] In order to find support for this supply side theory, I estimated equation (1) with measures of population density (of four year olds) interacted with state fixed effects and the age-eligibility terms. The positive relationship between population density and the effects of Universal Pre-K supports the thin markets story (Appendix Table 1).

[31] Each entry in these tables responds to the estimate of Universal Pre-K on the preschool enrollment of the children of a particular type of women. For example, in the sample the number of single women living in Oklahoma with less than a high school diploma who have a four year old child born within thirty days of September 1, 1995 is around one hundred.

level, but the difference between the two groups is not statistically significant. In Oklahoma, the effect of Universal Pre-K is positive for both single and married mothers, but the coefficient estimate of 6.6 percentage points is only statistically significant for married mothers. Here too though, I cannot rule out the possibility that the effects for both groups are the same.

Using the separate groups of married and single mothers with different levels of educational attainment, none of the coefficients for Oklahoma are statistically significant. However, in Georgia, there are statistically different effects for mothers of differing levels of educational attainment. Single mothers with less than a high school diploma and those with some college participation are 17 to 18 percentage points more likely to enroll their children in preschool because of Universal Pre-K availability. Married mothers with a high school diploma and those with some college participation also are more likely to enroll their four year olds in preschool because of Universal Pre-K availability. The enrollment response to the program by mothers with relatively low levels of education contrasts with the lack of discernable enrollment response for mothers with at least a Bachelor's Degree. As at least one quarter of the population of these mothers has obtained more than a Bachelor's Degree, it is unlikely that the lack of results for these women is due to small sample sizes. This pattern of seeing results for women with low levels educational attainment but none for women with higher levels of educational attainment is consistent with the theoretical model because more educated (higher wage) mothers are likely to be working and hence using a large amount of non-maternal child care even in the absence of Universal Pre-K.

Another dimension along which mothers deciding about child care differ is in the number of children (particularly young children) that they have. It is more expensive to place two children into care than one, but work is only possible if both children are cared for by others. I

estimate the effects of Universal Pre-K for the samples of married and single mothers with and without additional household children under age four (Appendix Table 2). The slight differences in the preschool enrollment responses of these different groups of women to Universal Pre-K availability are not statistically significant. I therefore conclude Universal Pre-K availability induced new preschool enrollment on average, though how much may have differed slightly over various segments of the population.

IV.c. Estimation Results for Maternal Labor Supply

Given that Universal Pre-K availability induced new preschool enrollment, the question becomes whether the increased enrollment served to alter the labor supply of mothers. As Table 5 shows, the effects of Universal Pre-K availability on the average labor supply of mothers statewide vary in sign and are not statistically significant.[32] For example, Universal Pre-K in Georgia increased mothers' probability of employment at any point in the previous year by 1.2 percentage points but the estimate is not statistically different from zero. A similar lack of effect is observed when hours or weeks worked is the dependent variable. Universal Pre-K programs have no effect on average maternal labor supply statewide. Universal Pre-K availability may have decreased the probability of public assistance receipt, however, by about 1 percentage point, a 20 percent decrease for this group of mothers. However, I am cautious about placing too much emphasis on these results because of the small sample sizes and vague definition of "public assistance receipt."[33]

[32] The estimates of the coefficients on demographic variables are generally of magnitudes and directions as predicted by economic theory and previous research. They are available from the author upon request.

[33] The actual Census questionnaire asks respondents to report the annual amount of "any public assistance or welfare payments from the state or local welfare office." It is not clear what this means to respondents and the answers to the question do not tell us anything about which type of public assistance the respondent is reporting (e.g. child care subsidies or food stamps).

Variation among women in preferences, wages and the price and quality of available child care, may affect labor supply responses to the introduction of Universal Pre-K. To determine whether this is the case, I again estimate equation (1) on different sub-samples of women defined in terms of demographic characteristics. First, I separate the sample based on the type of residential area (columns II through IV of Table 5). Many of the estimates of the effects of Universal Pre-K on maternal labor supply for women in different residential areas are statistically indistinguishable from zero. A notable exception is that Universal Pre-K increases employment of rural women in Georgia by 10 percentage points. This 20 percent increase in employment occurs whether I use employment in 1999 or in the week prior to the survey in 2000 as the dependent variable. Coupled with the dramatic increases in preschool enrollment in rural areas because of Universal Pre-K, this result suggests that the program had very different effects in these rural areas of the state than it did in more densely populated areas.

Next, I group women by marital status and education (Appendix Tables 3 and 4). Additionally, I examine separate groups based on marital status and whether the four year old was the mother's youngest child (Appendix Table 5). Although some mothers, particularly single mothers with no younger children, tend to increase their labor supply because of Universal Pre-K availability, most of the estimates are not precise enough to be distinguished statistically from zero.

IV.d. Effects of Preschool Participation on Maternal Labor Supply

Although the intention-to-treat effects reported in the last section were negligible, preschool use may have effects on the labor supply of mothers who enroll their children and no effect on those that do not. To determine if this is the case, I estimate the effects of the treatment

on the treated (in the range of the cutoff date) using the cutoff variables ($cutoff$, $GA \times cutoff$, $OK \times cutoff$) as instruments in two stage least squares (2SLS) estimation. The estimated effects can be thought of as the local average treatment effect of preschool enrollment on the labor supply of mothers with children born close to the state cutoff date for enrollment in kindergarten and preschool, based on the underlying assumption that having a child born before the state cutoff date does not affect maternal labor supply through anything other than preschool enrollment. In the 2SLS results presented in Table 6, there appear to be no effects of preschool enrollment on maternal labor supply.

IV.e. Specification Checks

One concern about the results I present above is that perhaps the quartic is not the appropriate polynomial size to use. To assuage concerns about over-fitting the data, Table 7 presents the results from the estimation of equation (1) on the sample of mothers of four year olds when the dependent variable is preschool enrollment, employment in 1999 and usual hours worked per week in 1999. Across the columns for each outcome the estimated equation varies in the degree of the polynomial in the *Days* function.[34] Although the estimated effect of Universal Pre-K is only statistically significant for preschool enrollment, the estimates of the program's effects on the outcomes are fairly robust to the various specifications of the *Days* function. This confirms the validity of the assumption that, close to the cutoff, there is little difference between mothers of children on either side of the cutoff.

Another concern is that the choice of width – the 30 day window on either side of the cutoff that I use to select the sample – influences the results. There is a tradeoff between making

[34] Table 7 shows results using polynomial sizes up to four, but results using polynomial sizes up to seven are qualitatively similar and available from the author upon request.

the sample window wider (thereby increasing the sample size) or making it narrower (making the groups on either side of the cutoff as comparable as possible). In the extreme case, it would be difficult to argue that there are huge differences between otherwise observationally identical women whose children are born one day apart. But unobservable differences are much more likely when comparing observationally similar mothers whose children are born in June versus December.[35] Table 8 displays results of regressions run on samples with different widths. The estimates illustrate the tradeoff between precision and comparability. As the width of the sample narrows, the standard errors (reported in parentheses) generally grow while the estimated effect sizes also grow (in absolute value). However, the changes in the estimated effects of Universal Pre-K are not appreciably different across the columns.

Another way to check the exogeneity of the treatment is to compare the regression results when controls are included and excluded. This comparison can be performed by contrasting the results reported in the top panel (which includes controls) and the bottom panel (which does not include controls) of Table 8. There are very few statistically significant differences in the results with any of the dependent variables shown. The same is true for outcomes not reported in the table.[36]

Examining the results when using a "placebo" cutoff additionally supports the conclusion that the estimated effects are the effects of Universal Pre-K rather than artifacts of the specifications. I have performed analyses using a variety of placebo cutoffs including setting the placebo at 75, 60, 50, 30, 25 and 10 days on either side of the actual cutoff; Table 9 includes some of these results. Additionally, I chose days of the year at random and used those as placebo cutoffs. The estimated coefficients are, for the most part, not significant. The exception occurs

[35] These differences across quarter of birth are highlighted in Bound, Jaeger, and Baker 1995; Bound and Jaeger 2000 and McCrary and Royer 2005.
[36] Results are available from the author upon request.

when the dependent variable is preschool enrollment and the placebo cutoff falls close to the actual cutoff. Because the effects of the cutoff are so strong, the placebo cutoff also appears to have an effect.

V. Conclusion

This paper has two key findings – first, that Universal Pre-K significantly and substantially raised preschool enrollment of four year olds in Georgia and Oklahoma, and second, that labor supply of mothers of four year olds in those states generally did not increase.

The availability of Universal Pre-K in Georgia and Oklahoma raised the preschool enrollment of four year olds by 12 to 15 percent statewide. Preschool enrollment rates of children residing in less densely populated residential areas are most affected by the availability of Universal Pre-K. The largest effects of the program are in rural areas, while smaller but still significant effects are found in larger and smaller urban areas. Also, the increases in enrollment are largest for children with mothers at the middle to lower end of the educational spectrum. These most affected groups increase preschool enrollment by about 25 percent because of Universal Pre-K availability. In contrast, women with at least a Bachelor's Degree are not estimated to have changed the preschool enrollment behavior of their children because of Universal Pre-K availability. Lastly, there are no differences in the increases of enrollment by marital status or whether mothers had other young children or not.

Although Universal Pre-K increases preschool enrollment in both states, the estimated effects on the labor supply of most mothers are statistically indistinguishable from zero. It does not appear that Universal Pre-K changes the labor supply of most mothers of four year olds (though it may have decreased their probability of receiving public assistance income) – with the

possible exception of women living in rural areas in Georgia, where the estimated enrollment effects were also largest. The lack of a program effect on maternal labor supply is somewhat surprising given the previous literature, which generally finds some statistically significant effects of universal care subsidization on labor supply. The reason may be that the population of women working has changed (compared to earlier studies which mostly focused on data from twenty to forty years ago) and therefore so has the population of women at the margin.[37] The findings in the recent literature that female labor supply elasticities have declined over time (Blau and Kahn 2007, Heim 2004) may explain these results. Another thing to note is that the effect estimated here is the effect of Universal Pre-K availability on maternal labor supply as compared to the early childhood preschool and child care subsidy landscape existing in 2000. As such, many women in the "control group" who are likely to be affected by child care subsidization, such as those from low income backgrounds, may already be receiving subsidies (such as Head Start) which would make it less likely that the universal programs would have any effect.

The combination of results – an increase in preschool enrollment coupled with little change in labor supply – signals that the return to the government's investment in Universal Pre-K should be measured by its effects on child outcomes. One potential explanation for the results would be a pattern of mothers of age-eligible children shifting from informal child care to formal child care. Because the Census asks about preschool, respondents likely do not answer affirmatively if children are enrolled at day care or are at a babysitter's or grandmother's house. If the results presented here are evidence of switching from informal to formal day care, the

[37] For example, the baseline rates of maternal employment in previous literature for women who had employment responses to universal subsidies were between 17 and 55 percent. The baseline employment rate for women in my analysis is about 77 percent. I note this difference as an important potential explanation for the differences between the results presented.

policy focus should be on the quality of Universal Pre-K relative to other existing modes of child care.

Finally, the results for mothers living in rural and less densely populated areas suggest Universal Pre-K increases access of some families to preschool. In future work, I plan to examine the supply side effects of the program in order to understand more about how Universal Pre-K affects the choices women make about child care, including choices between informal and formal care.

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Table 1: Kindergarten Cutoff Dates by State, 1999

State	*Cutoff Date*	*State*	*Cutoff Date*
Alabama	September 1	Montana	September 10
Alaska	August 15	Nebraska	October 15
Arizona	September 1	Nevada	September 30
Arkansas	August 1	New Hampshire	LEA
California	December 2	New Jersey	LEA
Colorado	LEA	New Mexico	September 1
Connecticut	January 1	New York	December 1
Delaware	August 31	North Carolina	October 16
Florida	September 1	North Dakota	August 31
Georgia	September 1	Ohio	September 30
Hawaii	December 31	Oklahoma	September 1
Idaho	September 1	Oregon	September 1
Illinois	LEA	Pennsylvania	LEA
Indiana	June 1	Rhode Island	December 31
Iowa	September 15	South Carolina	September 1
Kansas	August 31	South Dakota	September 1
Kentucky	October 1	Tennessee	September 30
Louisiana	September 30	Texas	September 1
Maine	October 15	Utah	September 1
Maryland	December 31	Vermont	January 1
Massachusetts	LEA	Virginia	September 30
Michigan	December 1	Washington	LEA
Minnesota	September 1	West Virginia	August 31
Mississippi	September 1	Wisconsin	September 1
Missouri	August 1	Wyoming	September 15

Note: Data on kindergarten cutoffs are from a survey conducted by the Indiana Department of Education, http://www.doe.state.in.us/legwatch/2000/a_kinder_issues.html (Accessed August 21, 2007.) LEA states are those that leave the designation of kindergarten age eligibility cutoffs to the local education authorities.

Table 2: Means of Demographic Characteristics

	GEORGIA		OKLAHOMA		OTHER STATES	
	Before Cutoff	*After Cutoff*	*Before Cutoff*	*After Cutoff*	*Before Cutoff*	*After Cutoff*
Age	31.500	31.488	30.680	30.978	32.127	32.176
	(0.248)	(0.237)	(0.303)	(0.344)	(0.048)	(0.046)
Age Squared	1027.322	1025.926	970.457	999.628	1067.919	1070.471
	(15.916)	(15.500)	(19.327)	(22.375)	(3.118)	(3.037)
White	0.587	0.585	0.754	0.754	0.710	0.719
	(0.020)	(0.019)	(0.025)	(0.023)	(0.004)	(0.004)
Married	0.735	0.727	0.772	0.753	0.766	0.766
	(0.018)	(0.017)	(0.024)	(0.023)	(0.003)	(0.003)
Other Household	0.889	0.861	0.863	0.864	0.922	0.918
Members, 18+ Years	(0.026)	(0.024)	(0.024)	(0.027)	(0.005)	(0.005)
Other Household	0.130	0.099	0.171	0.122	0.111	0.102
Members, 0-17 Years	(0.027)	(0.015)	(0.033)	(0.025)	(0.003)	(0.003)
Own & Step Children,	0.016	0.024	0.033	0.019	0.027	0.026
18+ Years	(0.005)	(0.006)	(0.013)	(0.007)	(0.002)	(0.001)
Own & Step Children,	0.139	0.147	0.147	0.184	0.165	0.159
13-17	(0.019)	(0.015)	(0.025)	(0.025)	(0.004)	(0.004)
Own & Step Children,	0.530	0.552	0.604	0.570	0.615	0.609
5-12	(0.028)	(0.027)	(0.043)	(0.041)	(0.006)	(0.006)
Own & Step Children,	0.279	0.328	0.305	0.328	0.304	0.311
0-3	(0.020)	(0.020)	(0.030)	(0.027)	(0.004)	(0.004)
High School Degree	0.260	0.279	0.287	0.298	0.255	0.256
	(0.018)	(0.017)	(0.025)	(0.024)	(0.003)	(0.003)
Some College	0.343	0.298	0.358	0.362	0.358	0.356
	(0.020)	(0.018)	(0.027)	(0.026)	(0.004)	(0.004)
BA Degree	0.189	0.194	0.199	0.178	0.180	0.183
	(0.016)	(0.016)	(0.023)	(0.022)	(0.003)	(0.003)
Graduate/Professional	0.082	0.088	***	***	0.074	0.073
Degree	(0.011)	(0.012)			(0.002)	(0.002)
Days	-199.6	-168.8	-199.9	-169.2	-231.4	-200.6
	(0.357)	(0.342)	(0.465)	(0.490)	(0.395)	(0.388)
Weighted N	7,989	8,885	3,013	3,410	199,490	207,894
Approximate N	1,000	1,100	600	700	30,000	31,000

Note: Based on the author's calculations using the Restricted Access Decennial Census Long Form Data. The sample includes mothers whose own singleton children were born within 30 days of the kindergarten cutoff in their state of residence and for whom data on all variables was available. *Days* measures the distance in days from a child's date of birth and March 15, 1995. *** means that the number could not be released from the data center due to confidentiality restrictions. Sample weights were used and standard errors are in parentheses. Standard deviations and actual sample sizes are waiting to be cleared from the Data Center but will be available from the author upon request.

Table 3: Estimates of the Effect of Universal Pre-K on Preschool Enrollment by Type of Residential Area

Explanatory Variable	ALL AREAS (I)	RURAL (II)	URBAN CLUSTER (III)	URBAN AREA (IV)
GA Cutoff	0.095	0.116	0.148	0.080
	(0.022)	(0.044)	(0.063)	(0.029)
OK Cutoff	0.062	0.099	0.104	0.002
	(0.031)	(0.048)	(0.070)	(0.052)
Cutoff	0.081	0.085	0.072	0.134
	(0.009)	(0.018)	(0.011)	(0.026)
Age	0.020	0.040	0.018	0.007
	(0.003)	(0.007)	(0.004)	(0.010)
(Age x 100) Squared	-0.022	-0.055	-0.018	-0.010
	(0.504)	(1.039)	(0.620)	(1.600)
White	-0.055	-0.137	-0.026	-0.074
	(0.005)	(0.014)	(0.006)	(0.017)
Married	-0.038	-0.052	-0.031	-0.006
	(0.007)	(0.016)	(0.008)	(0.021)
Other Household Members, 18+ Years	-0.028	-0.041	-0.027	-0.007
	(0.005)	(0.013)	(0.005)	(0.016)
Other Household Members, 0 to 17 Years	-0.029	-0.026	-0.027	-0.041
	(0.006)	(0.011)	(0.007)	(0.017)
Own & Step Children, 18+ Years	-0.055	-0.012	-0.063	-0.052
	(0.014)	(0.028)	(0.016)	(0.045)
Own & Step Children, 13 to 17	-0.053	-0.051	-0.052	-0.040
	(0.005)	(0.010)	(0.006)	(0.016)
Own & Step Children, 5 to 12	-0.031	-0.028	-0.031	-0.030
	(0.003)	(0.006)	(0.004)	(0.009)
Own & Step Children, 0 to 3	-0.037	-0.049	-0.031	-0.061
	(0.004)	(0.009)	(0.005)	(0.013)
High School Degree	0.058	0.073	0.061	0.028
	(0.007)	(0.015)	(0.008)	(0.020)
Some College	0.148	0.153	0.148	0.129
	(0.006)	(0.015)	(0.008)	(0.019)
BA Degree	0.224	0.203	0.224	0.186
	(0.007)	(0.016)	(0.008)	(0.022)
Graduate/Professional Degree	0.260	0.245	0.258	0.205
	(0.007)	(0.019)	(0.008)	(0.029)

Note: Based on the author's calculations using the Restricted Access 2000 Decennial Long Form Data. Each column in the table represents results from a separate regression. The estimation is of equation (1) from the text, with a quartic in age of the child (in days). Sample includes children born within 30 days of the kindergarten cutoff in their state of residence who live in the residential area noted at the top of the column. Demographic characteristics include those listed in Table 2. State fixed effects are used and sample weights are incorporated. The dependent variable is the child's enrollment in preschool. As such, probit estimation methods are used. The results presented are marginal effects calculated at the means of continuous variables and for a change in dummy variables from zero to one. Standard errors are in parentheses.

Table 4: Estimates of the Effects of Universal Pre-K on Preschool Enrollment by Marital Status and Maternal Education

Dependent Variable		ALL	NO HSD	HSD	SOME COLLEGE	BA or GRAD
		(I)	(II)	(III)	(IV)	(V)
				Single Mothers		
Preschool Enrollment	GA Cutoff	0.113	0.180	-0.008	0.174	0.081
		(0.044)	(0.093)	(0.085)	(0.057)	(0.129)
	OK Cutoff	0.044	0.144	-0.157	0.113	0.089
		(0.070)	(0.164)	(0.133)	(0.091)	(0.124)
				Married Mothers		
Preschool Enrollment	GA Cutoff	0.090	-0.020	0.113	0.188	0.042
		(0.026)	(0.073)	(0.054)	(0.038)	(0.041)
	OK Cutoff	0.066	0.075	0.104	0.056	0.022
		(0.035)	(0.102)	(0.066)	(0.057)	(0.058)

Note: Based on the author's calculations using the Restricted Access 2000 Decennial Long Form Data. Each column and row set in the table represents results from a separate regression. The estimation is of equation (1) from the text, with a quartic in age of the child (in days). Sample includes children born within 30 days of the kindergarten cutoff in their state of residence whose mothers are of the martial and educational attainment status indicated at the top of the column. Demographic characteristics include those listed in Table 2, where appropriate. State fixed effects are used and sample weights are incorporated. The dependent variable is the child's enrollment in preschool. As such, probit estimation methods are used. The results presented are marginal effects calculated at the means of continuous variables and for a change in dummy variables from zero to one. Standard errors are in parentheses.

Table 5: Estimates of the Effect of Universal Pre-K on Maternal Labor Supply and Public Assistance Receipt by Type of Residential Area

Dependent Variable		ALL AREAS	RURAL	URBAN CLUSTER	URBAN AREA
		(I)	(II)	(III)	(IV)
Worked Last Year	GA Cutoff	0.012	0.106	-0.029	-0.026
		(0.022)	(0.033)	(0.070)	(0.031)
	OK Cutoff	0.011	0.018	0.042	-0.017
		(0.031)	(0.046)	(0.061)	(0.053)
Worked Last Week	GA Cutoff	-0.012	0.093	0.064	-0.066
		(0.025)	(0.042)	(0.074)	(0.032)
	OK Cutoff	-0.001	0.013	0.006	-0.006
		(0.034)	(0.050)	(0.074)	(0.055)
Hours Worked per Week Last Year	GA Cutoff	-0.508	-0.400	-1.560	-0.269
		(0.617)	(1.090)	(1.845)	(0.822)
	OK Cutoff	-0.401	-1.741	2.847	-0.013
		(0.789)	(1.116)	(1.832)	(1.407)
Weeks Worked Last Year	GA Cutoff	0.707	0.618	0.884	0.651
		(0.754)	(1.335)	(2.312)	(1.000)
	OK Cutoff	-0.552	-0.370	-0.408	-1.055
		(0.965)	(1.367)	(2.294)	(1.711)
Wage & Salary Income Last Year	GA Cutoff	851	1792	1632	-5589
		(1278)	(1686)	(1984)	(2483
	OK Cutoff	676	-49	133	3763
		(1635)	(1727)	(3396)	(2465)
Received Welfare Income Last Year	GA Cutoff	-0.010	-0.005	-0.021	-0.008
		(0.004)	(0.008)	(0.008)	(0.006)
	OK Cutoff	-0.005	0.022	-0.017	-0.007
		(0.007)	(0.023)	(0.010)	(0.010)

Note: Based on the author's calculations using the Restricted Access 2000 Decennial Long Form Data. Each column and row set in the table represents results from a separate regression. The estimation is of equation (1) from the text, with a quartic in age of the child (in days). Sample includes children born within 30 days of the kindergarten cutoff in their state of residence who live in the residential area noted at the top of the column. Demographic characteristics include those listed in Table 2, where appropriate. State fixed effects are used and sample weights are incorporated. When the dependent variable is binary, probit estimation methods are used. The results presented are marginal effects calculated at the means of continuous variables and for a change in dummy variables from zero to one. Standard errors are in parentheses.

Table 6. Instrumental Variables Estimates of the Effect of Preschool on Maternal Labor Supply

Dependent Variable	(I)
Worked Last Year	0.036 (0.091)
Worked Last Week	-0.081 (0.099)
Hours Worked per Week Last Year	-1.055 (3.786)
Weeks Worked Last Year	-4.007 (4.524)
Wage & Salary Income Last Year	5281 (4517)
Received Welfare Income Last Year	-5281 (4517)
F Statistic for First Stage	21.53

Note: Based on the author's calculation using the Restricted Access 2000 Decennial Long Form Data. Each row in the table represents results from a separate regression. The estimation is a two-stage-least-squares approach using the variables $cutoff$, $gacutof$, $okcutoff$ as instruments for preschool. Sample includes children born within 30 days of the kindergarten cutoff in their state of residence. Demographic characteristics included in both stages are those listed in Table 2 plus a quartic in age of the child (in days). State fixed effects are used and sample weights are incorporated. Standard errors are in parentheses.

Table 7: Estimates of the Effects of Universal Pre-K on Preschool Enrollment, Work in 1999 and Usual Hours of Work per Week in 1999 when Varying the Polynomial of the Days Function

Dependent Variable	Preschool Enrollment				Worked Last Year				Usual Hours Worked Last Year			
	I	II	III	IV	V	VI	VII	VIII	IX	X	XI	XII
Explanatory Variables												
Days/100	0.063	0.140	0.130	-0.389	-0.006	-0.028	0.245	0.320	0.331	1.310	-3.160	-3.164
	(0.025)	(0.054)	(0.147)	(0.275)	(0.024)	(0.052)	(0.145)	(0.274)	(0.632)	(1.350)	(3.629)	(6.880)
$(Days/100)^2$		0.021	0.015	-0.564		-0.006	0.152	0.235		0.272	-2.335	-2.339
		(0.013)	(0.080)	(0.274)		(0.012)	(0.079)	(0.269)		(0.331)	(1.992)	(6.820)
$(Days/100)^3$			-0.001	-0.255			0.028	0.064			-0.468	-0.470
			(0.014)	(0.116)			(0.014)	(0.113)			(0.353)	(2.890)
$(Days/100)^4$				-0.038				0.005				0.000
				(0.017)				(0.017)				(0.428)
Cutoff	0.081	0.081	0.081	0.081	0.001	0.001	0.001	0.001	-0.129	-0.125	-0.129	-0.129
	(0.009)	(0.009)	(0.009)	(0.009)	(0.009)	(0.009)	(0.009)	(0.009)	(0.223)	(0.223)	(0.223)	(0.224)
GA Cutoff	0.102	0.099	0.099	0.095	0.007	0.008	0.011	0.012	-0.402	-0.452	-0.508	-0.508
	(0.022)	(0.022)	(0.022)	(0.022)	(0.022)	(0.022)	(0.022)	(0.022)	(0.611)	(0.614)	(0.615)	(0.617)
OK Cutoff	0.070	0.066	0.066	0.062	0.006	0.007	0.010	0.011	-0.297	-0.345	-0.401	-0.401
	(0.031)	(0.031)	(0.031)	(0.031)	(0.032)	(0.032)	(0.031)	(0.031)	(0.785)	(0.787)	(0.788)	(0.789)

Note: Based on the author's calculations using the Restricted Access 2000 Decennial Long Form Data. Each column in the table represents results from a separate regression. The estimation is of equation (1) from the text, with a varying polynomial in age of the child (in days). Sample includes children born within 30 days of the kindergarten cutoff in their state of residence. Demographic characteristics include those listed in Table 2. State fixed effects are used and sample weights are incorporated. Because the dependent variables are binary, probit estimation methods are used. The results presented are marginal effects calculated at the means of continuous variables and for a change in dummy variables from zero to one. Standard errors are in parentheses. A ** represents results unavailable because of small sample sizes.

Table 8. Estimates of the Effects of Universal Pre-K on Preschool Enrollment, Work in 1999 and Public Assistance Receipt in 1999 When Varying the Width of the Sample and the Inclusion of Controls

		(I)	(II)	(III)	(IV)	(V)	(VI)
		WHOLE SAMPLE	200 DAYS	100 DAYS	60 DAYS	30 DAYS	14 DAYS
Demographic Controls Included							
Preschool Enrollment	GA Cutoff	0.075	0.080	0.076	0.080	0.095	0.121
		(0.010)	(0.010)	(0.013)	(0.016)	(0.022)	(0.031)
	OK Cutoff	0.063	0.067	0.063	0.068	0.061	0.075
		(0.013)	(0.014)	(0.018)	(0.022)	(0.031)	(0.042)
Mother's Employment 1999	GA Cutoff	0.011	0.004	-0.003	0.004	0.013	0.024
		(0.010)	(0.010)	(0.013)	(0.016)	(0.022)	(0.032)
	OK Cutoff	0.008	0.004	0.008	0.003	0.012	0.036
		(0.013)	(0.014)	(0.018)	(0.022)	(0.031)	(0.041)
Public assistance receipt	GA Cutoff	-0.005	-0.004	-0.004	-0.006	-0.010	-0.013
		(0.002)	(0.003)	(0.004)	(0.004)	(0.005)	(0.005)
	OK Cutoff	0.000	0.001	0.005	-0.002	-0.005	0.001
		(0.004)	(0.004)	(0.006)	(0.006)	(0.007)	(0.012)
No Demographic Controls Included							
Preschool Enrollment	GA Cutoff	0.069	0.075	0.069	0.069	0.085	0.109
		(0.010)	(0.010)	(0.013)	(0.017)	(0.023)	(0.032)
	OK Cutoff	0.063	0.068	0.064	0.065	0.064	0.079
		(0.013)	(0.013)	(0.017)	(0.022)	(0.030)	(0.042)
Mother's Employment 1999	GA Cutoff	0.010	0.003	-0.005	0.000	0.011	0.002
		(0.010)	(0.010)	(0.013)	(0.016)	(0.022)	(0.033)
	OK Cutoff	0.007	0.004	0.007	-0.004	0.015	0.047
		(0.013)	(0.014)	(0.018)	(0.023)	(0.031)	(0.042)
Public assistance receipt	GA Cutoff	-0.006	-0.003	-0.002	-0.006	-0.014	-0.032
		(0.005)	(0.005)	(0.007)	(0.008)	(0.009)	(0.008)
	OK Cutoff	-0.002	-0.001	0.000	-0.012	-0.010	-0.004
		(0.007)	(0.007)	(0.009)	(0.010)	(0.014)	(0.020)

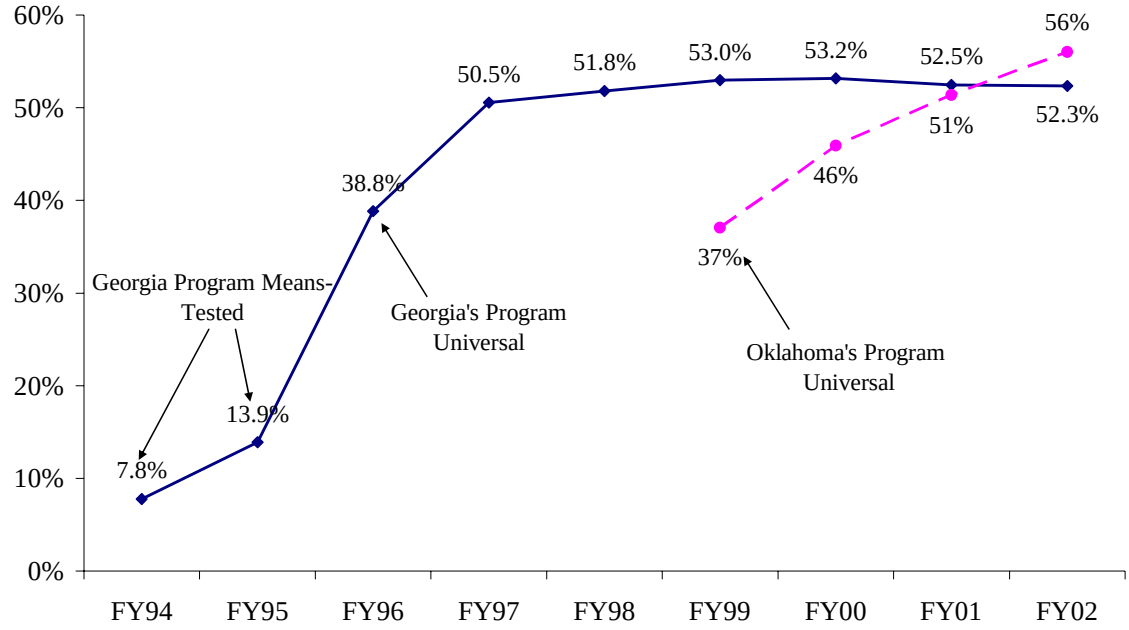
Notes: Based on the author's estimation of equation (1) using the restricted-access 2000 Decennial Long Form Data, with a quadratic in age of the child (in days). Each column and row set in the table represents results from a separate regression. Sample includes children born within the indicated number of days of the kindergarten cutoff in their state of residence. When included, demographic characteristics include those listed in Table 2 (upper panel). Because the dependent variables are binary, probit methods are used. Results presented are marginal effects calculated at the means of continuous variables and for a change in dummy variables from zero to one. Standard errors are in parentheses.

Table 9. Estimates of the Effects of Universal Pre-K on Preschool Enrollment, Work in 1999 and Public Assistance Receipt in 1999, When Using Placebo Cutoffs

Placebo Cutoff		(I) 75 Days After Actual	(II) 50 Days After Actual	(II) 25 Days After Actual	(III) Actual Cutoff	(IV) 25 Days Before Actual	(V) 50 Days Before Actual	(VI) 75 Days Before Actual
Preschool Enrollment	GA Cutoff	0.001	0.009	0.037	0.076	0.057	0.050	0.051
		(0.021)	(0.016)	(0.014)	(0.013)	(0.014)	(0.016)	(0.022)
	OK Cutoff	0.020	0.026	0.025	0.063	0.057	0.043	0.033
		(0.027)	(0.021)	(0.019)	(0.018)	(0.019)	(0.022)	(0.029)
Mother's Employment 1999	GA Cutoff	-0.023	-0.013	-0.013	-0.003	-0.008	0.009	-0.001
		(0.022)	(0.016)	(0.014)	(0.013)	(0.020)	(0.015)	(0.014)
	OK Cutoff	-0.036	0.014	0.015	0.008	0.025	-0.001	0.002
		(0.029)	(0.020)	(0.018)	(0.018)	(0.027)	(0.021)	(0.018)
Welfare Receipt	GA Cutoff	-0.003	-0.001	0.001	-0.004	0.008	0.000	-0.003
		(0.005)	(0.004)	(0.004)	(0.004)	(0.021)	(0.005)	(0.004)
	OK Cutoff	0.008	0.005	0.007	0.005	0.024	0.012	0.008
		(0.011)	(0.007)	(0.007)	(0.006)	(0.030)	(0.008)	(0.007)

Notes: Based on the author's calculation using the Restricted Access 2000 Decennial Long Form Data. Each column and row set in the table represents results from a separate regression. The estimation is of equation (1) from the text, with a quadratic in age of the child (in days). Sample includes children born within the 100 days of the kindergarten cutoff in their state of residence indicated at the top of the column. Demographic characteristics include those listed in Table 2. The cutoff variables used for Georgia and Oklahoma are set at the days identified by the column headings. Because the dependent variables are binary, probit estimation methods are used. The results presented are marginal effects calculated at the means of continuous variables and for a change in dummy variables from zero to one. Standard errors are in parentheses.

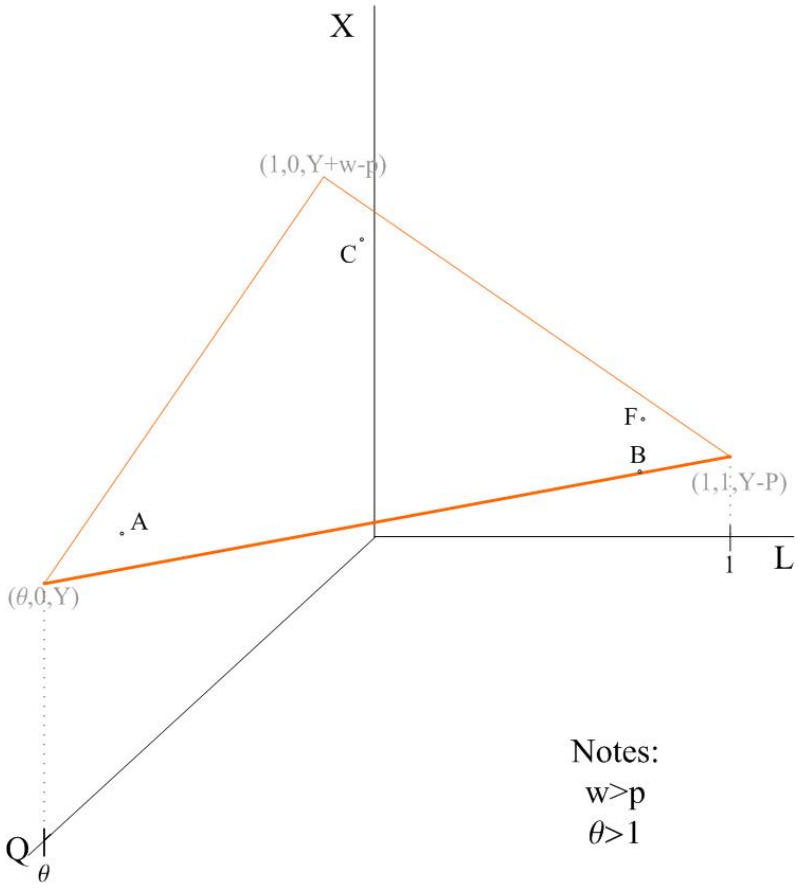
Figure 1. Percentage of Four Year Olds Enrolled in the Pre-K Programs



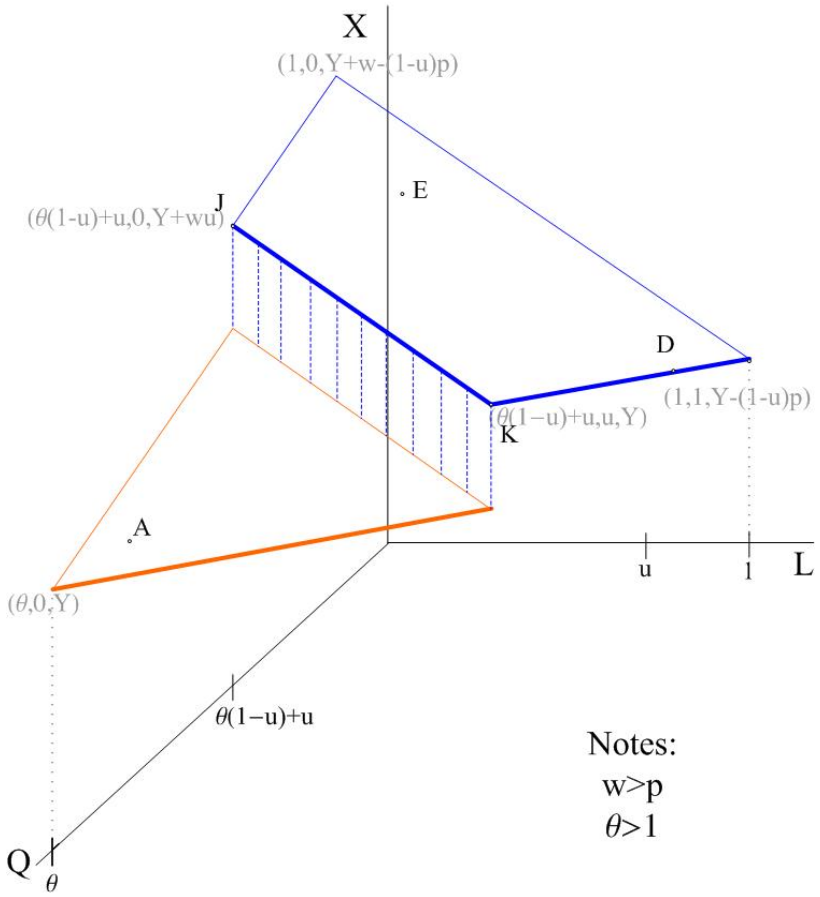
Notes: From Brackett, et al. (1999) and various web sources. A fiscal year runs from October of the previous year to September of the year in its name. For example, FY96 runs from October 1, 1995 to September 30, 1996. Percent of population of four year olds is calculated using the Census Bureau's Time Series of State Population Estimates by Age, which can be found at http://www.census.gov/.

Figure 2. Examples of Mother's Budget Frontiers With and Without Universal Pre-K

Panel A. Absence of Universal Pre-K



Panel B. Presence of Universal Pre-K



Note: See Section II.b. for model details. In brief, consumption is X, a mother's leisure is L, and the "quality" of her child care is determined by the amount of quality a child spends his mother and the amount of time he spends in the care of others, $Q = \theta C_m + C_o$. The mother has exogenous income Y and can earn an exogenous wage w per unit of time, while putting her child in child care costs p per unit of time.

Figure 3: Maternal Characteristics Around the Discontinuity

Figure 3.A. Percent of Mothers Who are White

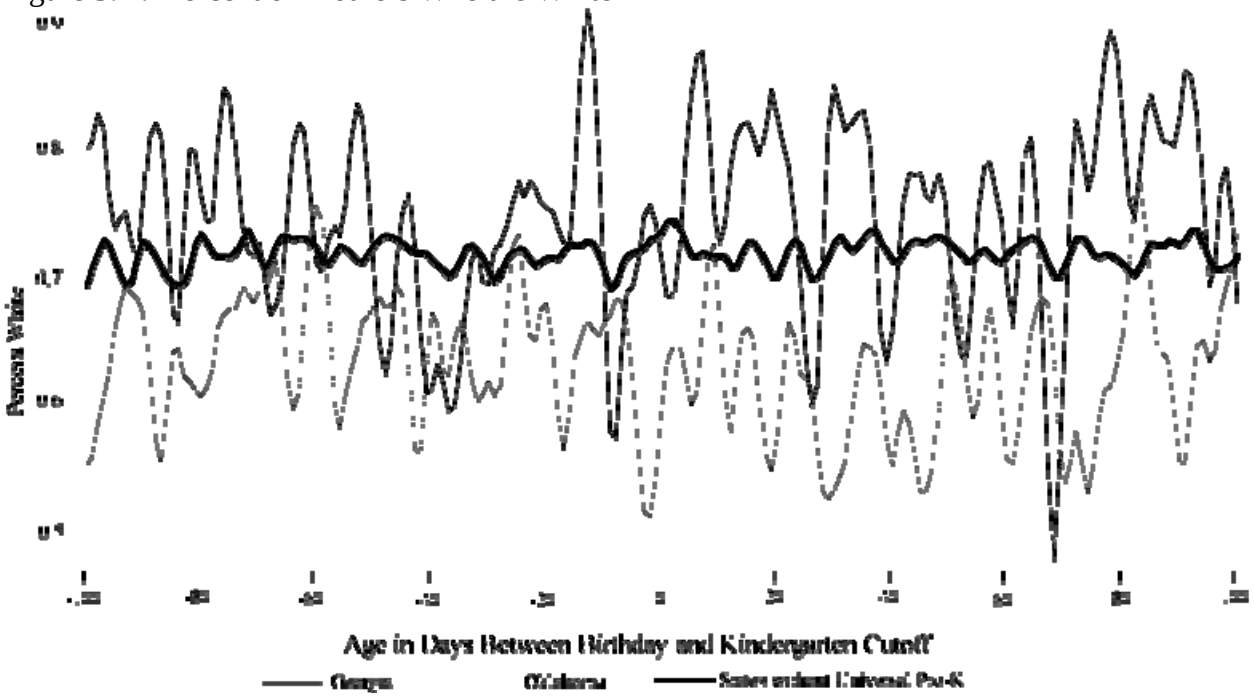


Figure 3.B. Percent of Mothers Who are Married

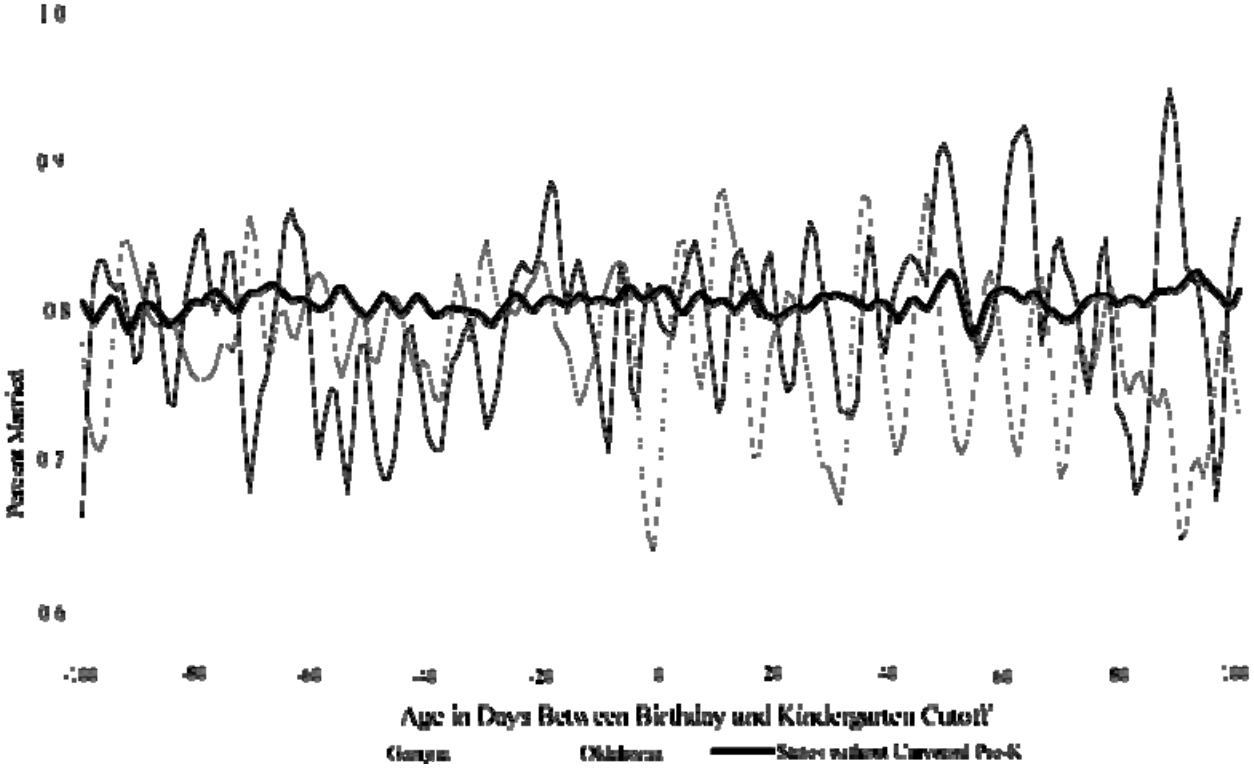


Figure 3.C. Age of Mothers

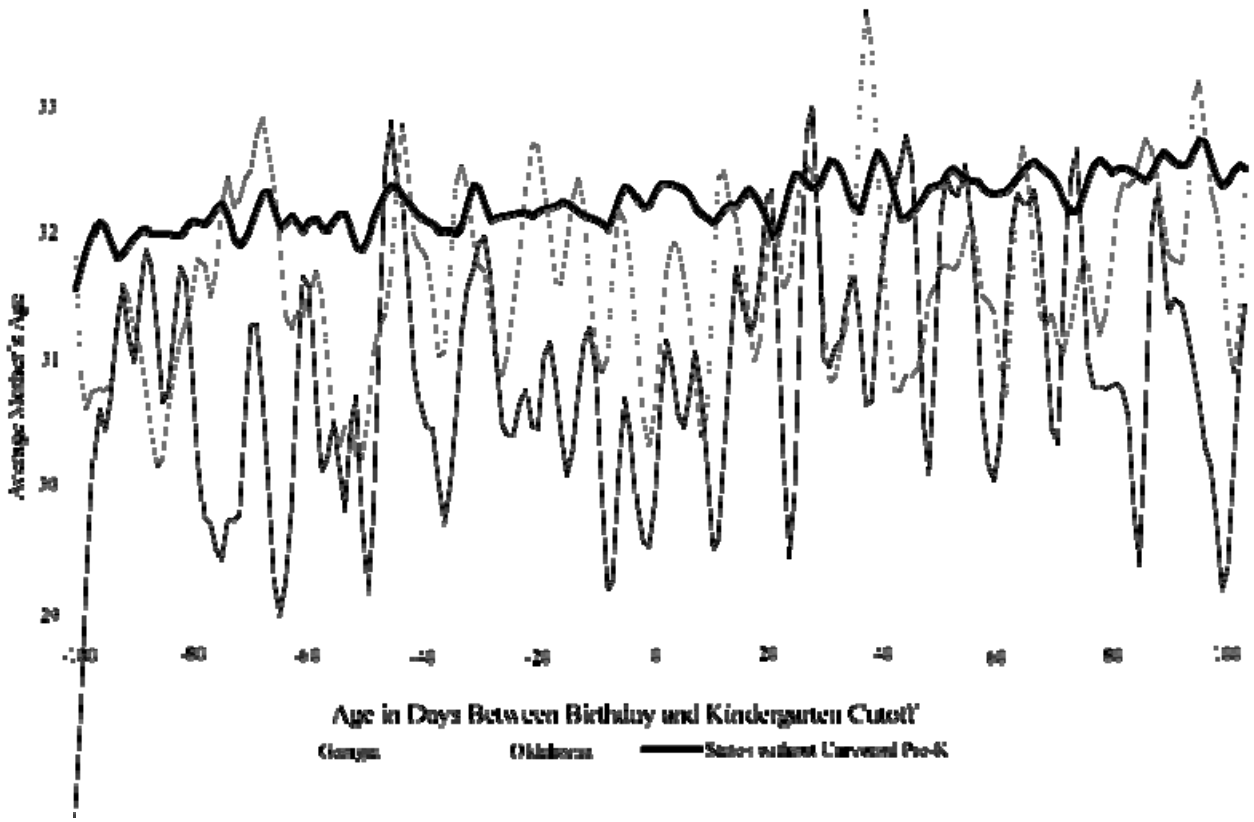


Figure 3.D. Percent of Mothers with Children Aged Zero to Three

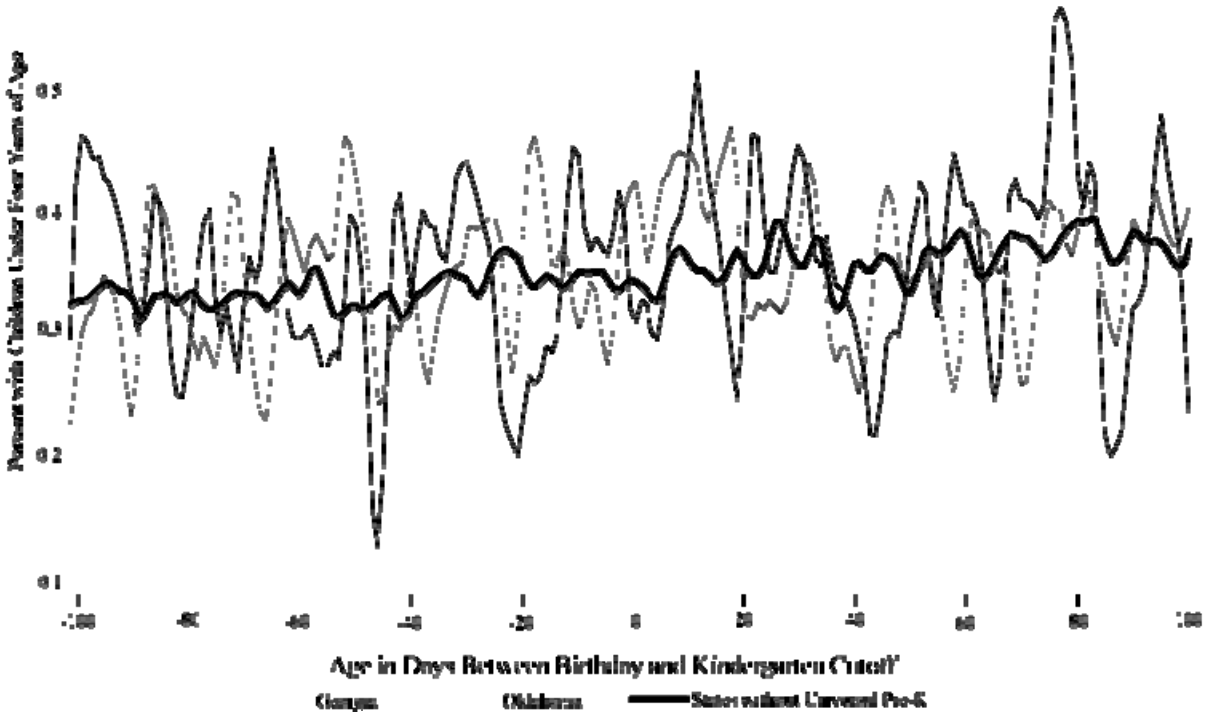
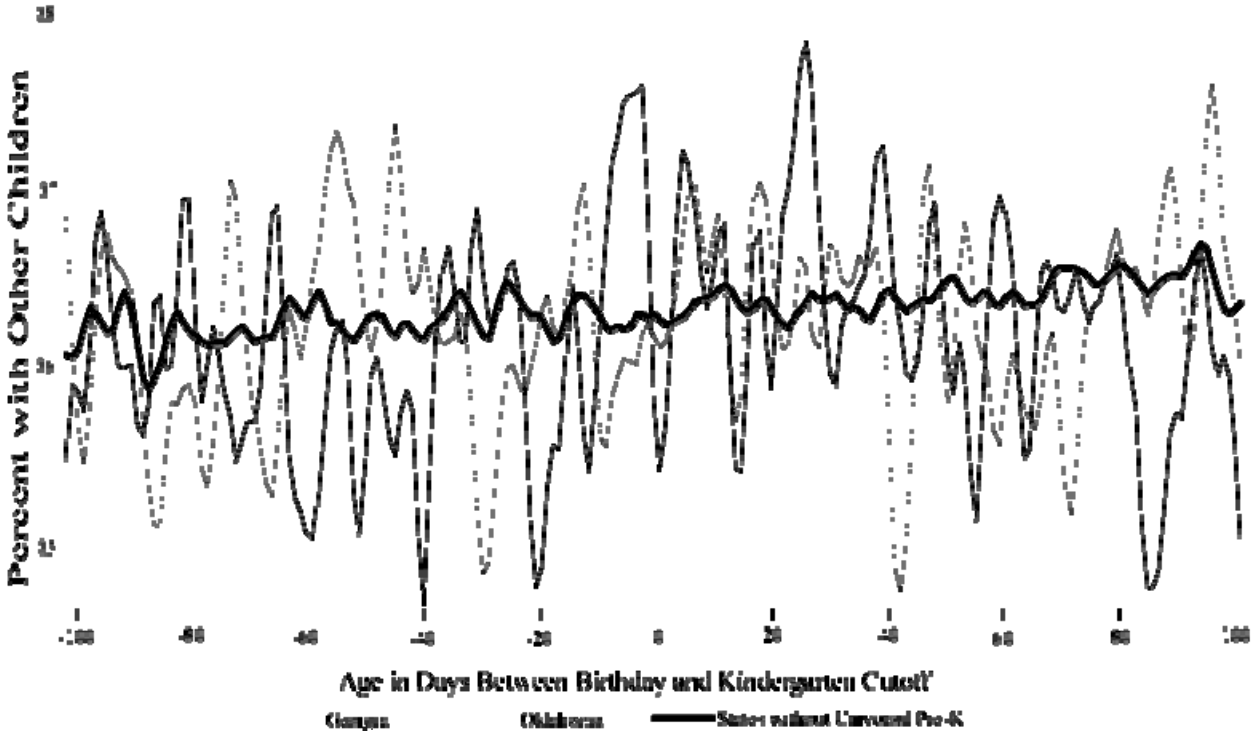
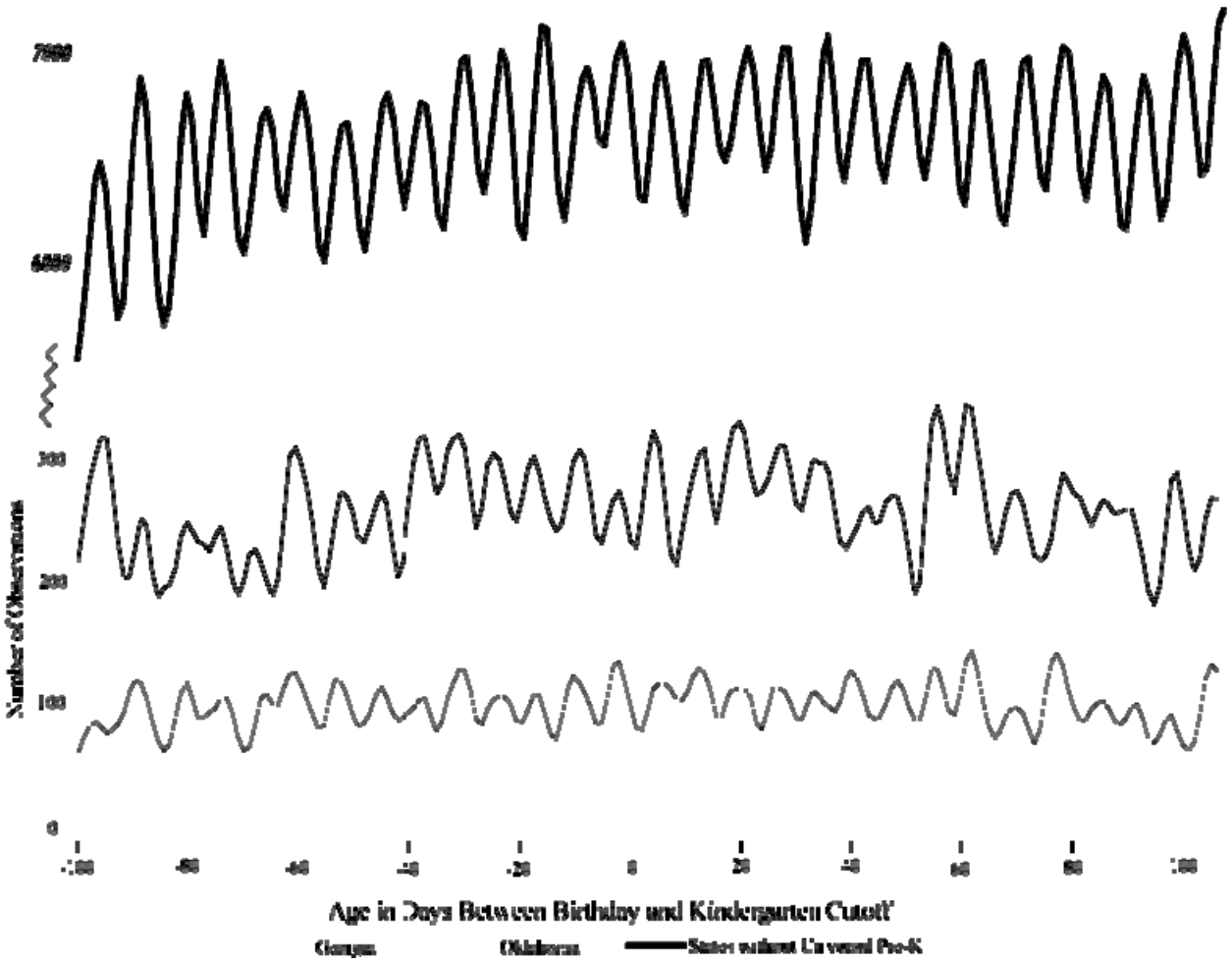


Figure 3.E. Percent of Mothers with Children Under 18



Note: Based on the author's calculations using Restricted Access 2000 Census Decennial Long Form Data.

Figure 4. Density of Observations Near the Cutoff



Note: Based on the author's calculations using Restricted Access 2000 Census Decennial Long Form Data.

Figure 5: Maternal Outcomes around the Cutoff

Figure 5.A. Preschool Enrollment Rate

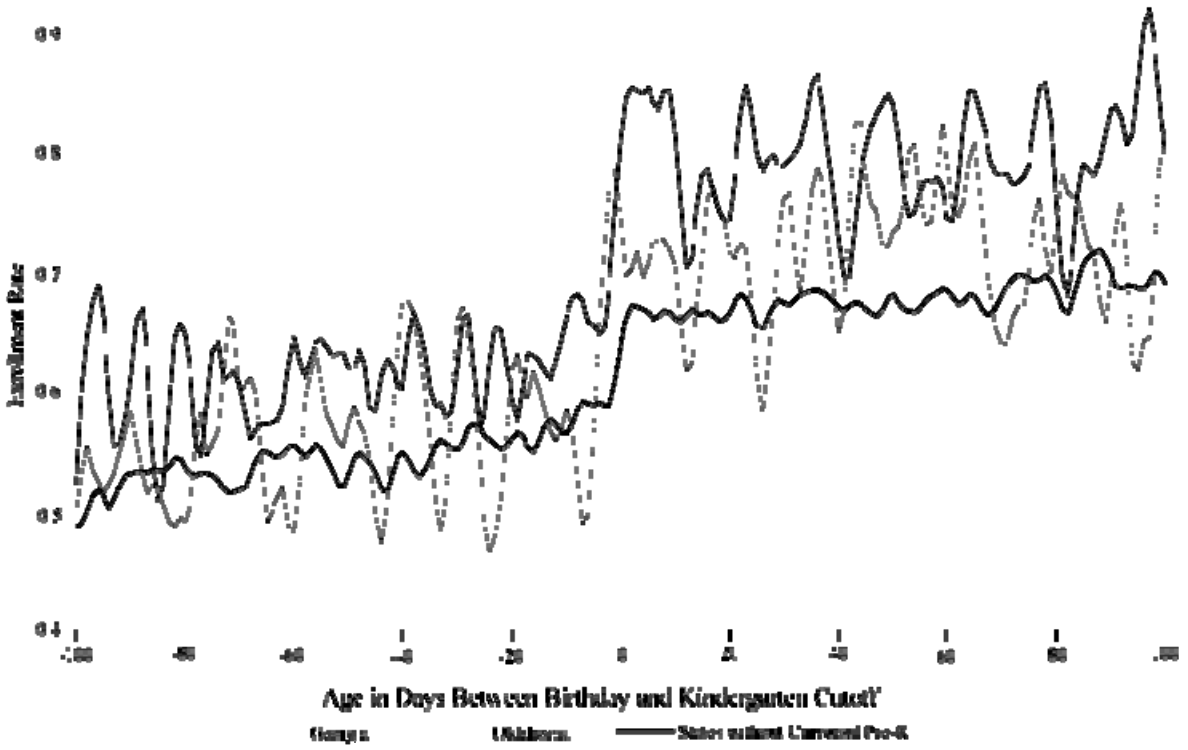
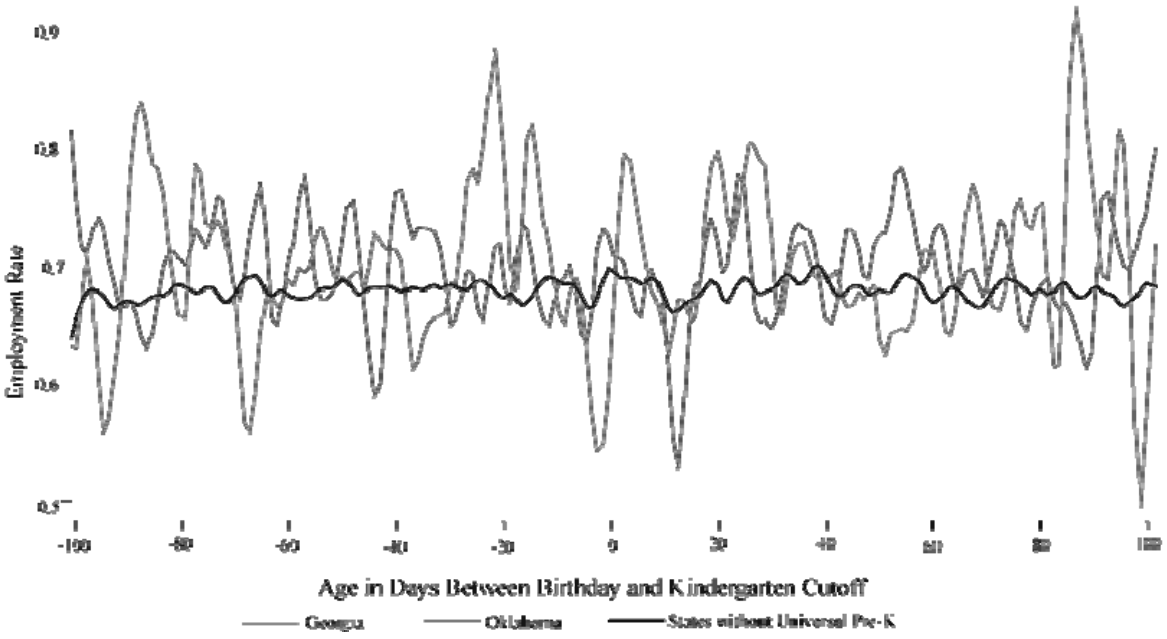
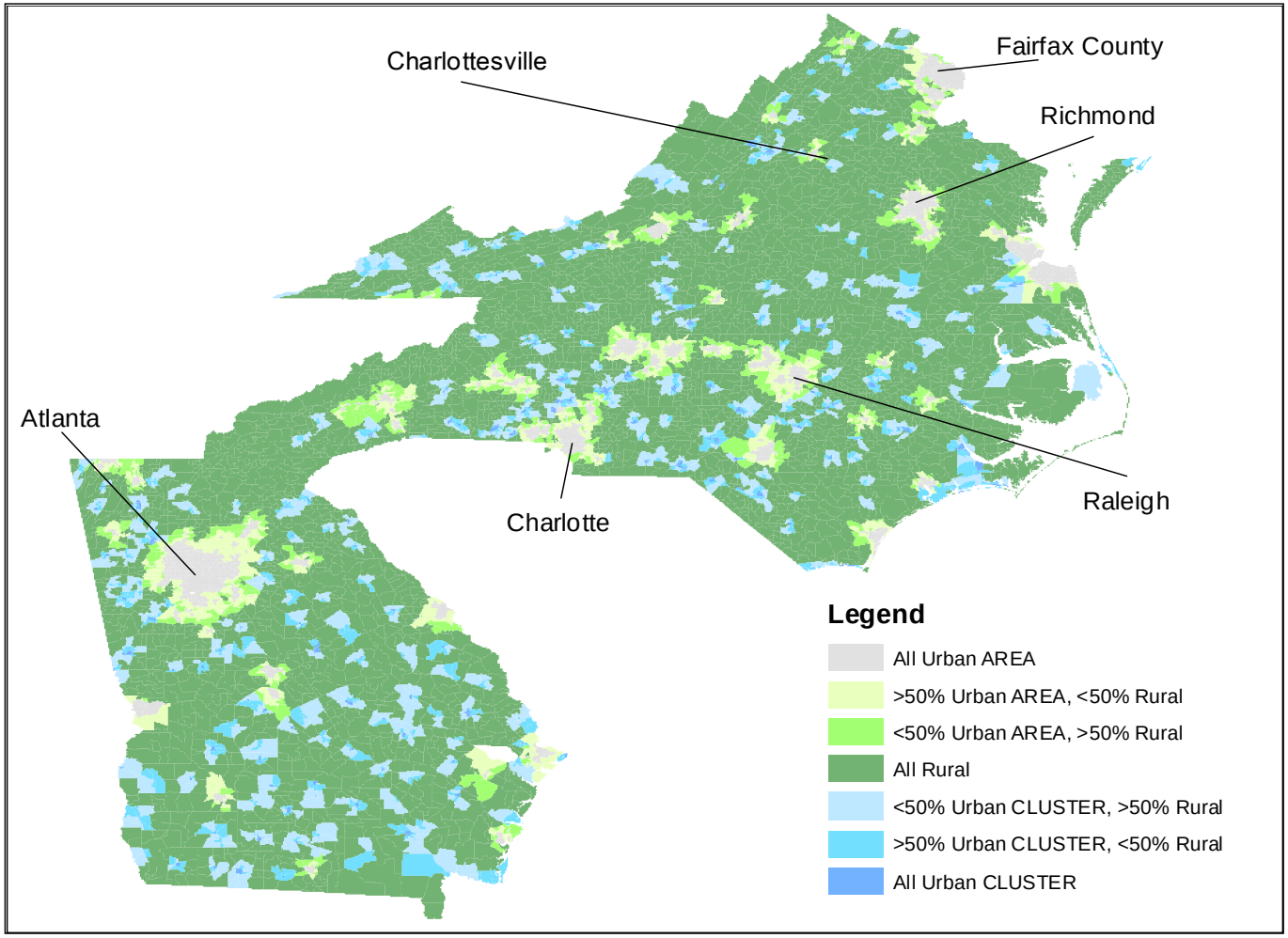


Figure 5.B. Employment Rate of Mothers in 1999



Note: Based on the author's calculations using Restricted Access 2000 Census Decennial Long Form Data.

Figure 6: Examples of the Census Bureau's Classification of Areas



Note: Based on the author's calculations using Census Summary File 1. The rural, urban cluster and urban area classifications are based on the Census Bureau's definitions.

Appendix.

I. Solution of the Motivating Theoretical Model

A simple static model of maternal labor supply and child care decisions motivates the empirical analysis in this paper. A mother has Cobb-Douglas preferences over family consumption (X), her leisure (L) and the "quality" of her child care (Q). She may spend her time in either work (H), leisure or care of her child (C_m). Normalizing the total amount of time to be one, her time constraint is $1 = H + L + C_m$. The child must be in someone's care at all times, either maternal or non-maternal (C_o), i.e. $1 = C_m + C_o$. Placing the child in the care of others for the period costs p. The mother can earn a market wage ($w > p$) if she spends the period working.[38] The mother has some exogenous endowment of income (Y), which includes the income of her husband if she is married. Normalizing the price of consumption to one, her budget constraint is $Y + wH = X + pC_o$. The total "quality" of child care is the sum of the amount of time spent with the mother and with others, weighted by the relative quality of maternal care, $Q = \theta C_m + C_o$.[39]

The mother's problem is therefore to chose her consumption, leisure and time with her child to maximize $X^{\alpha}Q^{\beta}L^{1-\alpha-\beta}$ subject to $Q = \theta C_m + C_o$; $Y + wH = X + pC_o$; $1 = H + L + C_m$; $1 = C_m + C_o$ and $1 \geq H \geq 0; 1 \geq L \geq 0; 1 \geq C_m \geq 0; 1 \geq C_o \geq 0; X \geq 0; 0 \leq \alpha \leq 1; 0 \leq \beta \leq 1; w > p.$ The Langrangian can be written down and solved for the following Kuhn-Tucker conditions, where λ is the Langrange multiplier:

i. $\frac{\alpha}{X} - \lambda_1 \leq 0;\ X \geq 0;\ X\left(\frac{\alpha}{X} - \lambda_1\right) = 0$

ii. $\frac{\beta(\theta-1)}{\theta C_m + 1 - C_m} - (w-p)\lambda_1 - \lambda_2 \leq 0;\ C_m \geq 0;\ C_m\left(\frac{\beta(\theta-1)}{\theta C_m + 1 - C_m} - (w-p)\lambda_1 - \lambda_2\right) = 0$

iii. $\frac{1-\alpha-\beta}{L} - w\lambda_1 - \lambda_3 \leq 0;\ L \geq 0;\ L\left(\frac{1-\alpha-\beta}{L} - w\lambda_1 - \lambda_3\right) = 0$

iv. $1 - C_m \geq 0;\ \lambda_2 \geq 0;\ \lambda_2(1 - C_m) = 0$

v. $1 - L \geq 0;\ \lambda_3 \geq 0;\ \lambda_3(1 - L) = 0$

Before the introduction of Universal Pre-K, the mother's demand equations at interior solution points, i.e. subject to the non-negativity constraints and bounds on the mother's time, are the following:

$$C_m^* = \frac{\beta Y}{w-p} + \frac{\beta\theta - 1}{\theta - 1};$$

$$X^* = \alpha Y + \alpha(w-p)\frac{\theta}{\theta-1};\quad L^* = \frac{(1-\alpha-\beta)Y}{w} + \frac{(1-\alpha-\beta)(w-p)}{w}\frac{\theta}{\theta-1}.$$

[38] The average price for a year of full-time center care for a four year old in Georgia is $5,243 (see Appendix), which translates to a per hour price of approximately $2.30. Allowing for the mother's wages to be less than or equal to the price of child care does not qualitatively change the results of the model.

[39] If θ is greater than one, maternal care is of superior quality to care provided by others.

The solution to her problem depends on the relative qualities of care and the balance between her wage and the price of child care by others. The greater a mother's wage compared to the price of child care the less time she spends with her child, *ceteris paribus,* because her time with the child has become more costly (relative to market time). Similarly, the higher the relative quality of the mother's care, *ceteris paribus,* the more time she spends in the care of her own child as leisure and employment become relatively more expensive.

Using the time constraint that requires the child to be cared for at all times, we can back out the amount of time the child spends in the care of others for women at an interior solution points, $C_o^* = \frac{\beta Y}{p-w} + \frac{(1-\beta)\theta}{\theta - 1}$. Similarly we can represent the optimal amount of maternal labor supply $H^* = 1 - \frac{\beta Y}{w-p} - \frac{\beta\theta - 1}{\theta - 1} - \frac{1-\alpha-\beta}{w} Y - \frac{(1-\alpha-\beta)(w-p)}{w}\frac{\theta}{\theta-1}$.

There are also potential corner solutions to the problem. These occur depending on the balance between the mother's wage and the price of child care and the mother's quality of care relative to the quality of care by others. Most of these women with $w > p$ have interior solutions. However, in one example, if a woman has no endowment the utility maximizing amount of time for her to spend with her child given the non-negativity constraints might be zero. In this case, C_m will be zero if $\theta < \frac{1}{\beta}$, which might occur if she is worse at providing care than the market and/or her preferences for high quality child care are not strong.

A Universal Pre-K program enters the model as a fixed amount of care by others (u) offered at no charge to the mother. For the sake of simplicity, I assume that the Universal Pre-K program offers the same quality as the care provided by others. Additional care by others continues to be available at the market price p per unit.

Universal Pre-K introduces income effects for some mothers in states where it is available.[40] Comparative statics for a change in exogenous income are:

$$\frac{dC_m^*}{dY} = \frac{\beta}{w-p} > 0; \quad \frac{dC_o^*}{dY} = \frac{\beta}{p-w} < 0; \frac{dX^*}{dY} = \alpha > 0; \quad \frac{dL^*}{dY} = \frac{(1-\alpha-\beta)}{w} > 0.$$

If time with the children is a normal good, she increases her own spent time with her child when her income increases. She also increases her consumption and leisure.

For some mothers, Universal Pre-K offers a price subsidy for child care. Comparative statics for a change in the price of child care are:

$$\frac{dC_m^*}{dp} = \frac{\beta Y}{(w-p)^2} > 0; \quad \frac{dC_o^*}{dp} = -\frac{\beta}{(p-w)^2} < 0; \frac{dX^*}{dp} = -\alpha\frac{\theta}{\theta-1}; \quad \frac{dL^*}{dp} = -\frac{1-\alpha-\beta}{w}\frac{\theta}{\theta-1}.$$

Noticeably, the direction of change of consumption and leisure in response to a change in price depends on the relative quality of the mother's care.

When Universal Pre-K causes a mother to spend less time with her children than a cash subsidy would, it forces her to a point along the segment FG. The relationship between a change in the mother's time with her own child and a change in the other ways a mother can spend her time is the following:

[40] It should be noted that because of the program's all-or-nothing nature, the change associated with Universal Pre-K is a fairly large discrete increase in income.

$$\frac{dL}{dC_m} = -\frac{(1-\alpha-\beta)}{(1-\beta)}\frac{(\omega-\pi)}{\omega}; \quad \frac{dX}{dC_m} = \frac{-\alpha}{(1-\beta)}(\omega-\pi).$$

These relationships are driven by the fact that a mother must spend her time either in work, leisure or time with her child. Therefore for each unit decrease in C_m, the mothers time in leisure increases by $\frac{(1-\alpha-\beta)}{(1-\beta)}\frac{(\omega-\pi)}{\omega}$ and the mother's time at work increases by $\frac{\alpha}{(1-\beta)}\frac{(\omega-\pi)}{\omega}$.

While the results from the estimation in this paper do not provide direct estimates of the parameters of this model, the data can give us some clues about underlying theoretical parameters. For example, in Georgia, fifteen percent of mothers of four year olds do not report sending their children to preschool or working. Assuming these mothers are better quality caregivers than market care, the tangency points of these mothers lie along the budget frontier for the group of mothers who do not take up the program. (I can not tell whether these mothers use zero child care by others or just less than u of child care by others.) It is likely that these women have high preferences for quality combined with low wages relative to the price of child care and an endowment from which to finance their consumption. More importantly, the results signal that the elasticity of maternal labor supply with respect to the price of child care is not large because mothers do not seem to respond to this large implicit child care subsidy by increasing their amount of time in the labor market.

Appendix Table 1: Effects of Universal Pre-Kindergarten on Preschool Enrollment by Population Density

Explanatory Variable	(I)
Cutoff	0.085
	(0.009)
Georgia Cutoff	0.115
	(0.029)
Oklahoma Cutoff	0.116
	(0.036)
Population Density	3.393
	(0.514)
Cutoff x Population Density	-1.645
	(0.676)
GA x Population Density	41.095
	(15.511)
GA x Cutoff x Population Density	-16.949
	(22.804)
OK x Population Density	75.534
	(39.567)
OK x Cutoff x Population Density	-113.899
	(55.881)

Note: Based on the author's calculations using the Restricted Access 2000 Decennial Long Form Data. The estimation is of equation (1) from the text, with a quartic in age of the child (in days). Sample includes children born within 30 days of the kindergarten cutoff in their state of residence. Demographic characteristics include those listed in Table 2. Population density is defined as the population of four year olds per square mile within one's county of residence multiplied by 1,000. State fixed effects are used and sample weights are incorporated. The dependent variable is the child's enrollment in preschool. As such, probit estimation methods are used. The results presented are marginal effects calculated at the means of continuous variables and for a change in dummy variables from zero to one. Standard errors are in parentheses.

Appendix Table 2: Estimates of the Effects of Universal Pre-K on Preschool Enrollment by Marital Status and Presence of Younger Children

		With Younger Children	With No Younger Children
		(I)	(II)
		Single Mothers	
Preschool Enrollment	GA Cutoff	0.132	0.103
		(0.088)	(0.051)
	OK Cutoff	0.106	0.010
		(0.128)	(0.084)
		Married Mothers	
Preschool Enrollment	GA Cutoff	0.052	0.113
		(0.046)	(0.030)
	OK Cutoff	0.065	0.064
		(0.062)	(0.042)

Note: Based on the author's calculations using the Restricted Access 2000 Decennial Long Form Data. The estimation is of equation (1) from the text, with a quartic in age of the child (in days). Sample includes children born within 30 days of the kindergarten cutoff in their state of residence whose mothers have the characteristics noted. Demographic characteristics include those listed in Table 2, where appropriate. State fixed effects are used and sample weights are incorporated. The dependent variable is the child's enrollment in preschool. As such, probit estimation methods are used. The results presented are marginal effects calculated at the means of continuous variables and for a change in dummy variables from zero to one. Standard errors are in parentheses.

Appendix Table 3: Estimates of the Effect of Universal Pre-K on Employment and Public Assistance Receipt of Single Women by Level of Educational Attainment

Dependent Variable		ALL (I)	NO HSD (II)	HSD (III)	SOME COLLEGE (IV)	BA or GRAD (V)
				Single Mothers		
Worked Last Year	GA Cutoff	0.051	0.108	0.030	0.042	**
		(0.031)	(0.083)	(0.056)	(0.043)	
	OK Cutoff	0.052	-0.119	0.147	0.047	**
		(0.047)	(0.178)	(0.031)	(0.048)	
Worked Last Week	GA Cutoff	0.029	-0.011	-0.003	0.123	0.018
		(0.049)	(0.106)	(0.082)	(0.063)	(0.102)
	OK Cutoff	0.014	0.189	-0.120	0.043	-0.232
		(0.075)	(0.165)	(0.130)	(0.107)	(0.261)
Hours Per Week Last Year	GA Cutoff	0.239	-1.589	-0.412	2.336	-0.490
		(0.989)	(2.427)	(1.464)	(1.712)	(3.657)
	OK Cutoff	1.204	-0.734	0.478	4.775	-3.062
		(1.373)	(4.010)	(1.974)	(2.376)	(4.372)
Weeks Worked Last Year	GA Cutoff	0.525	0.044	1.506	1.021	0.250
		(1.416)	(3.815)	(2.389)	(2.228)	(3.624)
	OK Cutoff	-1.888	-7.460	-3.239	1.802	0.102
		(1.966)	(6.303)	(3.222)	(3.093)	(4.333)
Wage & Salary Income Last Year	GA Cutoff	243	-808	-1132	879	3497
		(2420)	(4409)	(2885)	(2564)	(19564)
	OK Cutoff	-1223	-6908	-735	2165	-8093
		(3361)	(7285)	(3891)	(3560)	(23391)
Public assistance receipt	GA Cutoff	-0.059	-0.060	-0.116	0.008	**
		(0.033)	(0.099)	(0.038)	(0.062)	
	OK Cutoff	-0.045	-0.075	0.061	-0.089	**
		(0.050)	(0.165)	(0.118)	(0.026)	

Note: Based on the author's calculations using the Restricted Access 2000 Decennial Long Form Data. The estimation is of equation (1) from the text, with a quartic in age of the child (in days). Sample includes children born within 30 days of the kindergarten cutoff in their state of residence whose mothers have the characteristics noted. Demographic characteristics include those listed in Table 2, where appropriate. State fixed effects are used and sample weights are incorporated. When the dependent variable is binary, probit estimation methods are used. The results presented are marginal effects calculated at the means of continuous variables and for a change in dummy variables from zero to one. Standard errors are in parentheses. A ** represents results unavailable because of small sample sizes.

Appendix Table 4: Estimates of the Effect of Universal Pre-K on Employment and Public Assistance Receipt of Married Women by Level of Educational Attainment

		(I)	(II)	(III)	(IV)	(V)
Dependent Variable		ALL	NO HSD	HSD	SOME COLLEGE	BA or GRAD
				Married Mothers		
Worked Last Year	GA Cutoff	0.002	0.050	-0.024	0.006	0.002
		(0.026)	(0.074)	(0.055)	(0.045)	(0.043)
	OK Cutoff	-0.001	0.080	0.012	-0.017	-0.056
		(0.037)	(0.105)	(0.065)	(0.062)	(0.079)
Worked Last Week	GA Cutoff	-0.020	0.011	-0.074	0.032	-0.034
		(0.028)	(0.068)	(0.055)	(0.048)	(0.048)
	OK Cutoff	-0.003	-0.015	0.018	-0.068	0.069
		(0.038)	(0.093)	(0.067)	(0.065)	(0.073)
Hours Per Week Last Year	GA Cutoff	-0.649	0.794	-0.390	-3.089	1.076
		(0.750)	(2.083)	(1.401)	(1.316)	(1.437)
	OK Cutoff	-0.729	1.347	1.070	-1.166	-2.495
		(0.936)	(2.653)	(1.647)	(1.548)	(2.026)
Weeks Worked Last Year	GA Cutoff	0.885	2.018	0.624	1.110	0.724
		(0.887)	(3.179)	(1.820)	(1.535)	(1.459)
	OK Cutoff	-0.119	-1.182	2.763	-1.459	-0.533
		(1.106)	(4.049)	(2.140)	(1.806)	(2.057)
Wage & Salary Income Last Year	GA Cutoff	764	-359	552	-1130	3172
		(1499)	(3534)	(1862)	(2074)	(3882)
	OK Cutoff	1053	2774	1680	942	-833
		(1870)	(4500)	(2190)	(2440)	(5474)
Public Assistance Receipt	GA Cutoff	-0.004	-0.010	0.001	-0.006	**
		(0.004)	(0.031)	(0.019)	(0.006)	
	OK Cutoff	0.000	0.950	-0.019	0.006	**
		(0.007)	(0.003)	(0.004)	(0.019)	

Note: Based on the author's calculations using the Restricted Access 2000 Decennial Long Form Data. The estimation is of equation (1) from the text, with a quartic in age of the child (in days). Sample includes children born within 30 days of the kindergarten cutoff in their state of residence whose mothers have the characteristics noted. Demographic characteristics include those listed in Table 2, where appropriate. State fixed effects are used and sample weights are incorporated. When the dependent variable is binary, probit estimation methods are used. The results presented are marginal effects calculated at the means of continuous variables and for a change in dummy variables from zero to one. Standard errors are in parentheses. A ** represents results unavailable because of small sample sizes.

Appendix Table 5: Estimates of the Effect of Universal Pre-K on Employment and Public Assistance Receipt of Women b y Marital Status and Presence of Younger Children

		(I)	(II)	(III)	(IV)
		WITH YOUNGER CHILDREN	WITH NO YOUNGER CHILDREN	WITH YOUNGER CHILDREN	WITH NO YOUNGER CHILDREN
		Single Mothers		***Married Mothers***	
Worked Last Year	GA Cutoff	0.065	0.048	0.033	-0.015
		(0.068)	(0.033)	(0.046)	(0.033)
	OK Cutoff	-0.268	0.120	0.068	-0.030
		(0.168)	(0.019)	(0.064)	(0.045)
Worked Last Week	GA Cutoff	-0.042	0.036	-0.037	-0.008
		(0.023)	(0.055)	(0.048)	(0.035)
	OK Cutoff	0.032	0.038	0.035	-0.018
		(0.098)	(0.082)	(0.067)	(0.046)
Hours Per Week Last Year	GA Cutoff	-1.072	2.108	0.892	-0.302
		(0.683)	(1.726)	(1.608)	(1.136)
	OK Cutoff	1.803	4.113	1.882	-1.708
		(3.102)	(2.520)	(2.117)	(1.421)
Weeks Worked Last Year	GA Cutoff	-1.064	2.330	1.937	0.772
		(0.793)	(2.075)	(1.908)	(1.360)
	OK Cutoff	2.718	4.859	1.785	-1.426
		(3.602)	(3.029)	(2.513)	(1.702)
Wage & Salary Income Last Year	GA Cutoff	309.992	4079.370	2361.830	-122.402
		(671.357)	(2599.350)	(1789.700)	(1385.350)
	OK Cutoff	-6897.640	551.975	1485.190	-232.011
		(3048.305)	(3794.100)	(2356.390)	(1733.090)
Public Assistance Receipt	GA Cutoff	0.040	-0.051	-0.005	-0.004
		(0.020)	(0.035)	(0.007)	(0.005)
	OK Cutoff	-0.080	-0.028	0.105	-0.003
		(0.075)	(0.054)	(0.081)	(0.006)

Note: Based on the author's calculations using the Restricted Access 2000 Decennial Long Form Data. The estimation is of equation (1) from the text, with a quartic in age of the child (in days). Sample includes children born within 30 days of the kindergarten cutoff in their state of residence whose mothers have the characteristics noted. Demographic characteristics include those listed in Table 2, where appropriate. State fixed effects are used and sample weights are incorporated. When the dependent variable is binary, probit estimation methods are used. The results presented are marginal effects calculated at the means of continuous variables and for a change in dummy variables from zero to one. Standard errors are in parentheses. A ** represents results unavailable because of small sample sizes.